

THE PSYCHOLOGICAL REVIEW.

STUDIES FROM THE HARVARD PSYCHOLOGICAL LABORATORY. (II.)

COMMUNICATED BY PROFESSOR HUGO MÜNSTERBERG.

A. THE MOTOR POWER OF IDEAS.

BY HUGO MÜNSTERBERG AND W. W. CAMPBELL.

The direct dependence of bodily movements upon ideas is not only one of the most important, but also one of the most neglected, chapters of psychology. Its importance grows with the modern development of psychological theory. On the one hand, the tendency increases to consider the conscious phenomenon of will as a combination of elements which are not essentially different from the elements of ideas. Voluntary movements must also be understood as the results of sensations and feelings. On the other hand, the tendency is apparently increasing to emphasize the motor elements of ideas and to consider the motor effects of ideas as essential factors of their rôle in consciousness—especially to look at the phenomena of attention from this standpoint. Exact study of the direct relations between ideas and involuntary movements has therefore its bearing both on the intellectual and on the volitional side of the psychological life.

The most careful experiments made in this direction are on subjects in abnormal conditions, either hysterical or in the hypnotic state. In both cases the abnormal inhibitions make it possible that the movements under the influence of ideas take place without the interference of a conscious act of will. But it is obvious that these cases are extremely complicated and difficult to understand; above all, that the results obtained

from them cannot be identified directly with normal functions. With normal subjects the possibility of such experiments seems extremely limited. The resulting movement is here conscious, and the perception of it becomes therefore the motive for a voluntary impulse which changes or inhibits the movement, even when there is not a volition controlling the movement from the start. An action of the muscles under the influence of ideas without any interference of the will seemed open to quantitative experimental study only in the case of those movements which are too small to be perceived, as in the experiments on muscle-reading. Movements of the hand, which correspond to the ideas of space-direction, without a subjective knowledge of these movements themselves, can in such cases be registered graphically. Experiments on imitation, on time-measurements of muscular reaction, on coördination of movements, etc., present similar conditions. But all these methods do not offer more than general hints about the relations of ideas and movements. A quantitative study of the motor power of ideas, therefore, and the variations and conditions of this motor power, seemed impossible. Nevertheless, there is one group of muscles which allows just such experiments—the eye-muscles.

The facts on which our new method is based offered themselves almost by chance. A physicist informed one of the writers, Münsterberg, some years ago that he had discovered a proof that after-images are of central and not of peripheral origin. The proof was as follows: If he looked at a bright flame for twenty seconds, closed the eyes, and turned the head perhaps 45° , he then saw an after-image of the flame in the direction toward which the head was turned; but if he looked at the flame one second only, and closed the eyes and turned the head, he saw the after-image, not in this direction, but in the direction of the actual flame. He was of the opinion that this localization of the after-image, independent of the position of the head, could be the result of a central projection only. Münsterberg repeated the experiments and confirmed the result fully; but he saw immediately that the result is dependent entirely upon eye-movements. When he opened the eye-

lids after turning the head he found that the eyes were in the first case turned with the head, while in the second case they were directed towards the flame. The after-images appeared in the direction toward which the eyes were turned; the result had therefore no bearing on the question of the seat of after-images. But Münsterberg recognized that the experiment offers an instructive case of measurable muscle-action, under the influence of impressions, without conscious influence of the will, as the subject in both cases did not move the eyes voluntarily, but gave his whole attention to the head-movement only, while the eyes went their own way, either with the head or towards the flame. The meaning of the experiment, seen from this standpoint, is obvious. When I open my eyes and see a flame, this optical impression brings out the motor effect of fixating my eyes upon it—an effect which is the essential element in attention. When I turn my head with closed eyes this head-movement is naturally coördinated with the eye-movements, and the head-movement acts as stimulus to a motor reaction of the eyes. This second stimulus is, of course, the same whether I turn my head after twenty seconds or after one second. Now if this same stimulus brings out two so very different effects, it must be because the stimulus from the optical impression is different after twenty seconds and after one second. After one second the optical stimulus is stronger than the head-stimulus, and the eyes turn therefore to the flame; after twenty seconds the motor power of the optical stimulus is fully discharged, or at least so exhausted that it is overpowered by the head-stimulus, and the eyes follow the head. In this primitive form the experiment would tell us, of course, only that the motor power of the optical impression was after one second stronger than after twenty seconds. On the one hand the question arises: What was its strength during this interval? What after two, three, or ten seconds? Did it decrease continually, or is there a fluctuation or a sudden decrease? Are there periods in which neither stimulus is stronger, so that the eyes take a middle position, which perhaps by its changes may give a measurement of the phases of this motor power. On the other hand the question arises, What is the effect if we take other optical objects instead of the flame? Is the motor power

changed if I change the color of the flame, or if I take a picture or a figure or words, and what influence has the character of those objects? Of course a picture gives us no satisfactory after-image; but the after-image has nothing to do with the results which interest us. We can dispense with it, as it only serves as an index of the position of the eyes. If we open the eyes after turning the head, and state in what direction our eyes are looking while at the same time measuring the position of the head, we shall then be able to measure the motor power of the optical impressions by its dependence both upon the time we look on the objects and upon the character of the objects themselves. In this way we developed the systematic method upon which the following experiments are worked out.

One thing may be said at the outset: Not every one is able to repeat the experiment described above; and of those who do, not every one is a good subject for the more complicated experiments we are about to describe. The reasons for this are obvious. There are many people who are unable to turn the head voluntarily with closed eyes without first imagining in visual terms the direction in which the movement is made. This optical idea of course has the strongest influence upon the eyes, and the head only follows after the eyes. The idea to turn the head with closed eyes has on them the same effect as if they saw an object in indirect vision and in order to look at it directly make a head movement. It seems that most of the strong visualizers are of this kind, while the persons of motor type succeed easily, and after a little training get to be in a state in which the head is moved after an acoustical signal by a simple reflex which is without relation to visual presentations. We found that it was successful with almost half of the laboratory students. The fact that some are not skilful enough in fulfilling the conditions of the experiments, as they are unable to move the head with closed eyes without thinking of the optical space relations, does not of course signify anything against the method. Such persons often need only a short training to learn it. The following experiments were made with six subjects who all succeeded very well, but as a complete series was made only by three men, our report will be confined to the results of these three. One of them (Münsterberg) had

already had much practice in this kind of experiment and moved the head after a sound-signal as an acoustical reflex. The two others, Mr. Starbuck and Mr. W. W. Campbell, succeeded also from the beginning.

Our special question was, how the motor impulse to fixate the eyes upon an optical object varies with the quality of the impression and with the time of fixation, especially with regard to the intervals of 1, 2, 3, 4 seconds. Our point of view was in the first instance a methodological one: is the new method really fit to bring out characteristic and suggestive results? The apparatus used was as follows: A plane black surface was placed in a horizontal position, about the height of the chin of a person standing. Fixed perpendicularly upon this surface was a semicircle of black cloth 50 cm high, having a radius of 60 cm; the centre of the head of the standing subject was the centre of this semicircle, and at the height of his eyes were numbers marking the 180 degrees, by fives, on the lower part of this semicircle. A black easel with an opening 10 cm square was so constructed that cards or pictures could be fixed behind the easel so that they just filled the little window in the black frame; this easel stood in the middle of the semicircle just opposite the face of the subject, 60 cm from his eyes.

We had ten groups of optical stimuli, in each group ten different pieces; so that every one made 100 experiments without repetition of the stimulus. As a full series included 400 experiments, which were extended over a whole year, no stimulus was repeated during a period of several weeks; of course with the exception of special experiments on the influence of repetition, of which we shall speak later, they are not contained in these 400. The 10 groups were the following, the order here given being arbitrary, as they had to come quite irregularly during the experiments.

(1) Black single letter or number, 2 to 4 cm high, pasted upon white cardboard which filled the aperture. (2) Single word of five to eight printed letters, always a substantive, on white cardboard. (3) Card with nine such printed words, three lines of three words; each a substantive; the height of letters 1 cm. (4) Single pictures; these were black printed simple outline drawings of single objects, as a top, or bird, or

horse; the picture about 2 cm square on white cardboard. (5) Card with nine such pictures, three lines of three, arranged in a solid square. (6) Single color; it consisted of a piece of saturated colored paper, filling the full size of the aperture in the black frame. (7) Two or three color-strips, of different colors, each taking a half or a third of the space in regular form. (8) Irregular arrangement of different pieces of four or more different colors. (9) Photographs, cabinet size, heads of men and women. (10) Columns of several numbers of three places which were to be added by the subject.

The second part of the apparatus was an arrangement of two sound-signals, two electric hammers with different sounds, connected with Schumann's instrument for the study of the time-sense. This instrument consists of five wheels on one axle, which runs in the kymograph. Each wheel has movable platinum points, which can close a mercury contact on a special board. The rapidity of the kymograph and the distance of the five platinum points were so arranged during all our experiments that the contact of the second wheel was one second, of the next two, of the following three, and of the fifth wheel four seconds after the contact of the first wheel. The contact of the first wheel produced the sound of the first electric hammer, which was the signal for opening the eyes; the contact of one of the other four wheels produced the noise of the second hammer, which was the signal for closing the eyes and moving the head to the side. A pressure of the buttons decided which of the four wheels was to give the contact. In this way we had four exact intervals at our disposal.

The method of the experiment was as follows: The subject stood upright with closed eyes, his chin over the centre of the semicircle and his face in the direction of the middle of the semicircle before him. In order that this position might be found when the eyes were closed two symmetrical supports were provided for the hands. Some one object from the hundred stimuli was then placed in the black frame, directly in front of the subject. The noise of the distant kymograph gives the signal that the attention must be given to the experiment; four seconds later comes the first signal. The subject opens the eyes and looks at the object before him; after an

interval comes the second sound—the subject closes the eyes and turns the head to the right or left side and opens the eyes immediately. We both found it easier to turn always to the right, Starbuck turned alternately to right or left. The degree of the semicircle is then noted which the eyes of the subject fixate, and the direction of the head, which can be easily found by a wire pointer fixed on the forehead. Both the points for the eyes and for the head were noted to five degrees. The head movement and the opening of the eyes are therefore the same for all experiments; the difference lies only in the 100 stimuli and in the four intervals of time. A full series contained 10 experiments (corresponding to the 10 objects) in each of the 10 groups with each of the four intervals, that is, 400 experiments, made in entirely irregular order. When the subject opened the eyes to look at the window he never knew whether there would be color, word, picture, or numbers, etc., and never knew whether he had one, two, three, or four seconds to look. The following results give for each of the 40 groups the averages from 10 experiments. The 10 experiments of one group were always made on 10 different days, and all experiments under the same conditions. The degrees are counted from the point in front, 0° , toward the right, 90° ; the average head position is in parenthesis.

MÜNSTERBERG.

	1 Sec.	2 Sec.	3 Sec.	4 Sec.	Average.
1. Letters.....	12 (50)	25 (48)	48 (52)	48 (50)	33 (50)
2. One word.....	13 (46)	25 (46)	47 (47)	47 (52)	33 (48)
3. Nine words.....	0 (48)	8 (49)	17 (48)	28 (50)	13 (49)
4. Picture	14 (46)	32 (50)	45 (52)	47 (47)	35 (49)
5. Nine pictures.....	0 (45)	0 (47)	2 (50)	16 (49)	5 (48)
6. One color.....	4 (48)	28 (47)	31 (50)	47 (52)	28 (49)
7. Two colors.....	4 (47)	22 (47)	23 (46)	46 (48)	24 (47)
8. Irregular colors.....	0 (46)	16 (48)	25 (50)	39 (51)	20 (49)
9. Photograph.....	5 (52)	15 (51)	22 (52)	28 (50)	18 (51)
10. Numbers for adding	42 (52)	47 (47)	42 (52)	44 (49)	44 (50)
Average.....	9.4 (48.0)	21.8 (48.0)	30.2 (49.9)	39.0 (49.8)	25.1 (48.9)

It seems to us that no one who looks over these figures can regard them as resulting from chance. The head movement varied only between 46° and 52° , and nevertheless we see that the eyes turned unintentionally to any position between 0 and

50°, obviously dependent upon the quality of stimulus and the time-interval. The results prove that this method yields results which allow the finest discriminations of differences which can be studied in no other way. The eye-muscles work here with the exactitude of a physiological nerve-muscle preparation, the contractions of which correspond to the quality and intensity of the electric stimulus; so our eye-muscles show in the most perfect way the effect of the central motor impulse which the optical stimulus and its associations produce.

With regard to the details there is no result so constant as the increase of the eye-angles with the increasing time. With practically the same head-movement of 48° to 50°, the eyes remain on 9° after 1 sec., 22° after 2 sec., 30° after 3 sec., and 39° after 4 sec. The longer the time the weaker the motor stimulus which tends to turn the closed eyes in the direction of the optical object. And the average result of all the 10 groups holds for each of them with the exception of the tenth, which has a different character, as we shall see. This result, that the motor power of every impression is strongest after 1 sec. and decreases steadily, is characteristic for this subject only; the other tables show a very different result. The decrease is quickest where the impression is simple; for one color after 1 sec. 4°, after 2 sec. 28°; and it therefore reaches its maximum for one letter, one word, or one picture after 3 sec. The decrease is slowest where the act of reading or looking fills nearly the whole four seconds; thus the nine pictures have after 4 sec. the power to turn the eyes to an average of 16°. As stated, the only exception is the 10th group—the adding of figures: here the eyes turn even after 1 sec. almost as far as the head, and all four intervals give the same result as if the figures had no motor power at all. The notes on the results of self-observation, which were taken regularly, show that the opposite is true. The motor power of these figures, together with the associational ideas of adding, was evidently too great to allow the reflex head-movement. The subject felt himself unable to make the head-movement in the usual reflex way so long as he really was adding; the adding inhibited the movement. He was therefore in almost every case obliged to stop the adding intentionally after hearing the signal and to give his whole

interest to the movement, so that the idea of the calculation was quite swept out of consciousness. The tenth group can therefore not be compared for this subject with the nine others. If we take the average of the four intervals for those nine groups only, we get the exhaustion of the motor power still more distinctly: 5.8, 19.0, 27.8, 38.4.

If we compare those nine groups without regard to the influence of time, we find the following order: The weakest motor power results from one picture (35), one word (33), one letter (33), one saturated color a little stronger (28), two colors still stronger (24), then the irregular colors (20), the photograph (18); and strongest from the nine words (13) and the nine pictures (5). It is interesting to see that one letter and one word show exactly the same type of motor power, corresponding to the well-known fact that the time to apperceive them is the same. Very characteristic is the increase of motor energy produced by the variety of color; one color and two colors have the same motor intensity after one second, but even after two seconds the energy of one color is more discharged than that of two colors, and an irregular combination of many colors is stronger from the beginning. But the note-books show a fact which disappears in these general averages—that there are marked differences for the different colors; the red and yellow colors have more power than blue, etc.

The following tables give the results with the two other subjects:

STARBUCK.

	1 Sec.	2 Sec.	3 Sec.	4 Sec.	Average.
Letter.....	22 (36)	20 (33)	21 (35)	24 (32)	22 (34)
Word.....	14 (38)	15 (35)	25 (31)	23 (37)	20 (35)
Nine words.....	18 (37)	10 (41)	13 (39)	17 (33)	14 (37)
Picture.....	15 (38)	16 (37)	18 (37)	23 (34)	18 (36)
Nine pictures.....	12 (34)	9 (34)	16 (39)	13 (39)	12 (36)
Color	25 (37)	27 (35)	26 (35)	32 (36)	27 (36)
Two colors.....	22 (34)	21 (37)	24 (34)	16 (36)	21 (35)
Irregular colors....	17 (36)	14 (36)	2 (35)	8 (34)	10 (35)
Photograph.....	11 (37)	4 (41)	15 (38)	13 (38)	11 (38)
Figures.....	18 (33)	12 (37)	19 (37)	10 (38)	15 (36)
Average.....	17.4 (36.0)	14.8 (36.6)	17.9 (36.0)	17.9 (35.7)	17.0 (36.1)

CAMPBELL.

	1 Sec.	2 Sec.	3 Sec.	4 Sec.	Average.
1. Letter.	41 (41)	42 (42)	40 (40)	37 (44)	40 (42)
2. Word.	34 (40)	37 (43)	37 (43)	37 (43)	36 (42)
3. Nine words.	19 (45)	9 (46)	8 (43)	3 (43)	10 (44)
4. Picture.	36 (41)	28 (40)	28 (43)	29 (40)	30 (41)
5. Nine pictures.	7 (42)	15 (43)	6 (44)	3 (42)	8 (43)
6. Color.	33 (41)	37 (46)	22 (43)	35 (44)	32 (43)
7. Two colors.	21 (42)	18 (40)	27 (41)	27 (44)	23 (42)
8. Irregular colors.	16 (42)	14 (42)	20 (44)	9 (41)	15 (42)
9. Photograph.	30 (42)	9 (46)	8 (43)	3 (43)	12 (43)
10. Figures.	30 (40)	7 (42)	8 (40)	10 (44)	14 (41)
	26.7 (41.6)	21.6 (43.0)	20.4 (42.4)	19.3 (42.8)	22.25 (42.5)

It is evident that not only the absolute figures, but also the whole type of these two tables is different from the first one; but it is at the same time clear that here also the results are not due to chance. The difference of the results does not speak against the method; on the contrary, these tables prove that individual differences, which could not be stated in any other way, can be easily found by this method. It is of small importance, to be sure, that the average angle of head-movements is different for the three persons; the average for all experiments being, for M. 48.9° , for St. 36.1° , for C. 42.5° ; and, somewhat proportionately, the average eye-movement, for M. 25.1° , for St. 17.0° , for C. 22.25° . These absolute values are of course unimportant, as they depend upon the voluntary impulse to turn the head; the tables show that they were fairly constant for each subject. More characteristic by far is the difference with regard to the influence of time. With M. there was a continual decrease of the motor energy of the optical stimulus, the averages being, after 1 sec. 9.4, after 2 sec. 21.8, after 3 sec. 30.2, after 4 sec. 39.0. With St. it is quite different. His average is after 1 sec. 17.4, after 2 sec. 14.8, after 3 sec. 17.9, after 4 sec. 17.9. This means that the motor power of the optical impression has not reached its maximum in the first second, but is increasing, coming to its height after 2 sec., and even then not discharging itself, but going back only to the intensity of the first second and remaining there till the end of the fourth. But this general average of the ten groups loses just the characteristic details which the full table shows. We

see with St., firstly, that the motor energy may be also exhausted after the first second if the object is extremely simple—for instance, only one word or one picture or one color; secondly, that for more interesting objects the motor power fluctuates, getting, for instance, for added figures or nine pictures a second increase after four seconds; and thirdly, that it does not get the maximum for objects which are difficult to apperceive, like the irregular color combinations, before the third second. With Campbell the general average shows a continuous increase of motor energy; the increase is a slight one for the average of all ten groups, but looks very different when we consider the relations of the special groups. The table shows that the simple objects, as one word, one letter, one picture, one color, had from the beginning almost no motor influence at all, and did not get it later; only for the one color does the motor energy seem to increase in the third second. The nine pictures alone worked as a strong motor impulse from the start, decreasing a little in the second, but strongly increasing again in the third and fourth seconds. The nine words are steadily increasing; but it is especially characteristic that here, and much stronger still for the photographs and the added figures, the whole energy awakes during the second second, from 30 to 9, 8, 3 for the photograph, from 30 to 7, 8, 10 for the figures. All these complicated stimuli work in the first moment with Campbell like a meaningless object, and only when the associations awake does the motor energy increase rapidly. Even with the single picture there is the slight increase from 36 to 28. The type of the decrease and increase of motor energy is then extremely different for the three subjects: a steady decrease almost without exception for M., an increase till the beginning of the third second and a fluctuation during the following period for St., and a steady increase for C.

Much greater and almost complete agreement exists, however, for the three subjects if we abstract from the influence of time and look on the relative differences of the ten groups of impressions. All three subjects agree that a simple letter, word, color, or picture has the weakest motor influence; all agree that two colors have more motor power than one, and the

irregular colors still more (M., one color 28, two colors 24, irregul. col. 20; St., 27, 21, 10; C., 32, 23, 15); that nine pictures have by far stronger motor energy than one (M. 35-5, St. 18-12, C. 30-8); nine words stronger than one (M. 33-13, St. 20-14, C. 36-10); that the photograph of a person has far stronger motor function than the simply sketched picture of an object of daily life (M., picture 35, photograph 18; St., 18, 11; C., 30, 12), etc.

We have not yet mentioned one group, the eleventh of our experiments, which was made with the other ten—the group of repetitions. Every subject had sometimes for each of the four intervals, not a new optical stimulus, but that of the foregoing experiment with the regular pause of a few minutes only. The subject, of course, did not know that the stimulus would be repeated—he expected something new; and as these repetitions were seldom tried, there was in every case a decided feeling of unexpected acquaintance. We repeated especially those stimuli of which the first impression showed rather strong motor energy. The average of the first column represents, therefore, a greater motor influence than the average of all ten groups together. The average on both sides is for each subject from exactly the same forty stimuli.

	M.		St.		C.	
	First Time.	Second Time.	First Time.	Second Time.	First Time.	Second Time.
One sec.....	5 (45)	27 (41)	15 (39)	28 (40)	20 (46)	37 (45)
Two sec.....	8 (50)	40 (50)	9 (38)	26 (33)	21 (43)	34 (41)
Three sec.....	18 (50)	40 (48)	13 (40)	25 (38)	13 (46)	40 (40)
Four sec.....	22 (50)	47 (51)	14 (38)	23 (34)	17 (43)	37 (43)

It is obvious that, without exception, the same stimulus after the same interval has much weaker motor energy in the case of repetition; and this is true not only for these averages, but for every single trial.

The results of all the experiments show that the new method gives an answer to the three questions; the influence of the quality of the stimulus upon the intensity of the motor discharge, the influence of the duration of the stimulus, and the influence of the repetition of the stimulus. In all these cases the facts gave the most delicate record of individual differences. If we consider that this motor energy pro-

duced by a stimulus is the essential factor of that complicated emotional state which we call attention, it becomes evident that the whole question of the psychophysics of attention—its intensity, its fluctuations, etc.—is here opened to a method of study which frees us from the doubtful and narrow study of just perceivable sensations, and which allows an endless variation from the simplest optical sensations to the highest functions suggested by any optical impressions. Further, the mechanism of automatic impulses gets a method of exact study, which allows us to analyze those individual differences which even in our tables come out so decidedly, and which seem extremely important for the understanding of differences in central mental processes.

B. MEMORY. (II.)

BY JOHN BIGHAM, PH.D.

The following experiments are an immediate continuation of those published in this volume of this REVIEW, pp. 34-38. Their purpose is again to get empirical material for understanding the mechanism and the conditions of reproduction and memory. The special question is the influence of the time-interval between learning and recollecting with regard to its length and its filling. The experiments were made with six subjects during the winter 1893-94; average age of the subjects, 25.5 years.

The apparatus for all these experiments consisted of ten different classes of series, each series being composed of ten single presentations. Five of the classes were of visible presentations and five of audible. The visible series, placed horizontally on a white field, were exposed simultaneously by lifting a screen, twenty seconds for each series, and filled a space 40 cm long. The audible series were of course given in succession, 20 sec. being allowed for the ten presentations. The ten classes of series were:

1. Visible numbers: Zero and the nine digits, black, mounted upon white cardboard 3 cm square.
2. Audible numbers: The name of the ten numbers spoken by the experimenter.
3. Visible colors: Small squares of colored paper,

3 cm; white, gray, black, red, orange, yellow, green, blue, violet, brown. 4. Audible colors: The names of these ten colors spoken by the experimenter. 5. Visible forms: Ten geometrical figures drawn with red ink upon white cardboard squares, 3 cm. Star, cross, square, line, circle, etc. 6. Audible forms: The ten corresponding words spoken by the experimenter. 7. Visible words: Several hundred different monosyllabic words—nouns, verbs, and adjectives—each composed of two consonants and an intervening vowel; small black letters mounted upon white cardboard. These were arranged into series of ten words, the same word occurring only once for each subject during a period of at least a month, and then only in new combinations. Care was taken to secure proper variety in the sequence of the vowels and consonants of the words in each series. In the first six classes only the location of the single presentation in the series need be remembered; here, as every word occurred practically only once in all the experiments, the content of the presentation itself had to be remembered. The same is true for the three following contents. 8. Audible words: The same words, but arranged in series differing entirely from the visible series, and practically new words, as care was taken that every word appeared only once in a period of four or five weeks. The words were spoken by the experimenter. 9. Visible nonsense-syllables: Several hundred syllables, each consisting of two consonants with an intervening vowel; small black letters mounted like the words. The same method of arrangement as for the words. 10. Audible nonsense-syllables: The same method as with the audible words.

All the audible presentations were pronounced in a monotone without rhythm. For the visible numbers, colors, and forms, the observers had duplicates of the given presentations and arranged them as recollected. For the audible numbers, colors, and forms the observers were supplied with small white cardboard squares on which the names of the colors, forms, and numbers were written. For the words and syllables the subjects wrote the recollected letters upon strips of paper.

The errors were recorded by the conductor of the work and the subjects were not informed of their extent or charac-

ter. They were considered as misplacements or as omissions of the single presentations, and separate records were made of the two kinds of error. For the words and syllables there was of course a third kind of error possible—the introduction of words or syllables which did not exist in the objective series at all. The character of each series was fully described before it was presented for learning. The time required for recollection was recorded by a stop-watch. Fatigue was minimized by giving the observers a rest after each series. They were advised not to use any mnemonic devices and to give equal attention to every presentation in the series; but their methods of learning, remembering, and arranging the series were free from any special control. The subject was alone with the experimenter in the room; after experiments lasting some minutes he left the room and another subject came in his place.

The first question studied was the influence of the length of vacant time-intervals between the hearing and recollecting. The intervals examined were 2, 10, and 30 seconds; each of the ten classes was submitted to these three intervals. The subjects' eyes were closed during all the intervals and during the learning of the audible series. The series were never voluntarily remembered during the intervals. For each of the ten contents two series were used with each of the three intervals, with each of the six men, before proceeding to the next content, and these 360 experiments were repeated after an interval of some weeks spent in other researches, etc. The averages represent, therefore, a very similar degree of training.

Viewing the data of all ten classes in a general way, the following errors appear for the three unfilled intervals:

	M.	N.	P.	R.	S.	W.	Av.
2 sec.....	14.3%	34.8%	16.3%	30.5%	27.5%	27.5%	25.2%
10 sec.....	14.8	38.5	17.0	31.8	30.3	40.5	28.8
30 sec.....	18.3	44.5	20.5	34.3	31.3	38.1	31.1

The longer the unfilled interval between learning and recollecting, the weaker is the memory, with the one exception that W. has the greatest percentage of errors after 10 sec. We were unable to study the effect of longer intervals, as even 60 sec.—an interval which passes quite comfortably when filled—appears

extremely tedious without filling, and the resulting emotions interfere with the memory processes. If we disregard the visible or audible modes of presentations, and so reduce the ten classes to five, for which the conditions are exactly the same, we get the following averages for the six subjects:

	Numbers.	Colors.	Forms.	Words.	Syllables.
2 sec.....	10.0%	13.5%	18.8%	34.4%	49.2%
10 sec.....	7.3	20.2	23.5	36.3	56.7
30 sec.....	8.6	22.1	23.1	41.7	39.4
Average.....	8.6	18.5	21.4	30.5	48.3

In all the intervals the memory is increasingly weaker for numbers, colors, forms, words, syllables—the only exception being the syllables at 30 sec. The large increase in error for the two last groups is due to the greater difficulty with presentations, which do not recur in the various series. The lack of associations explains the fact that the nonsense-syllables are much harder to remember than words. It is remarkable that the numbers are remembered best after 10, worst after 2 sec., and that the syllables are remembered very much better after 30 sec. than after 2 or 10 sec.

If we separate the different kinds of error we find:

	Misplacing.	Forgetting.
2 sec.....	8.1%	14.9%
10 sec.....	10.8	16.2
30 sec.....	11.5	17.5

Both are correspondingly increased with the interval. The third kind of error, to which the words and syllables only were liable, the intrusion of 'alien' presentations, is:

	Words.	Syllables.
2 sec.....	0.4%	1.0%
10 sec.....	0.3	1.5
30 sec.....	0.4	1.7

A further analysis shows that misplacements are much more and increasingly common for numbers, colors, and forms, but less for words and least for syllables:

Numbers.	Colors.	Forms.	Words.	Syllables.
7.0	16.5	18.3	5.7	4.3

Forgetting, on the other hand, is much commoner with the words and is most frequent with the syllables :

Numbers.	Colors.	Forms.	Words.	Syllables.
1.6	2.1	3.5	29.9	43.2

As the syllables were in structure exactly like the words,—two consonants and an intervening vowel,—the difference in error indicates the relative superiority of association bonds for the verbal series. The same factor explains why the introduction of 'alien' syllables is more than three times as frequent as with the words.

The following data give the location of the three kinds of error. The location of the misplacements in the ten places is :

Place...	1	2	3	4	5	6	7	8	9	10
2 sec...	2.5	7.5	11.7	12.5	15.7	12.1	10.8	11.3	6.3	2.5
10 sec...	4.2	8.8	12.1	12.9	15.0	16.7	12.9	11.7	10.0	3.3
30 sec...	6.3	6.7	12.1	15.9	15.4	17.9	13.8	12.1	10.0	5.0
Average.	4.3	7.6	12.0	13.6	15.4	15.6	12.5	11.6	8.9	6.2

The location of forgetting :

Place...	1	2	3	4	5	6	7	8	9	10
2 sec...	5.8	11.3	15.9	23.8	20.4	14.6	23.8	14.6	12.1	6.7
10 sec...	9.2	12.5	19.6	23.3	24.6	19.6	20.4	14.6	12.5	6.3
30 sec...	7.9	19.6	21.3	24.2	21.3	19.6	26.3	15.0	10.4	10.0
Average.	7.6	14.3	18.8	23.6	22.1	17.7	23.7	14.7	11.6	7.6

For all intervals the misplacements are greatest at the fifth and sixth and least at the first and tenth places. Forgetting follows similar variations, but is greatest at the fourth and seventh places. The location of the wrong words or syllables is very different :

Place...	1	2	3	4	5	6	7	8	9	10
2 sec...	3.3	1.3	1.7	2.1	1.3	0.4	0.8	1.3	0.8	0.8
10 sec...	2.9	3.3	2.1	0.8	2.1	0.0	1.3	2.5	2.1	1.7
30 sec...	2.5	2.5	2.9	2.9	0.8	1.7	2.1	1.7	2.1	2.5
Average.	2.9	2.4	2.2	1.9	1.4	0.7	1.4	1.8	1.7	1.7

The errors are here most numerous at the beginning and are least in the middle.

We recorded, as mentioned, the time from the moment when the signal was given for reproducing the series learned till the moment when the subjective content of the memory was discharged. The average time for all observers is for one series of any ten presentations:

After 2 sec.....	45.4 sec.
After 10 sec.....	47.2 sec.
After 30 sec.....	48.8 sec.

The longer the interim, the longer the time for recollection. That means also; the longer the time for recollection, the larger the number of errors. This result is brought out still more distinctly by the individual records. If we place in order the six subjects with regard to the length of time necessary for recollection and with regard to the number of errors for all their experiments, we find:

2 sec.	{ Time :	M	S	P	W	R	N
	{ Error :	M	P	S	W	R	N
10 sec.	{ Time :	M	P	S	W	R	N
	{ Error :	M	P	S	R	W	N
30 sec.	{ Time :	M	P	S	W	R	N
	{ Error :	M	P	S	R	W	N

The foregoing correspondence proves, and later tables will show again, *that the memory which acts quicker acts better*; the number of errors increases regularly with the time used for recollection—a result which seems to be surprising and has no doubt an interesting bearing on pedagogical applications to memory. In the same way the time for recollecting corresponds to the number of errors with regard to the different contents. The order we found for the errors was: numbers, colors, forms, words, syllables. The time for them was:

Numbers.	Colors.	Forms.	Words.	Syllables.
26.6 sec.	32.5 sec.	43.2 sec.	74.7 sec.	81.9 sec.

Unfilled intervals represent a rare and artificial condition for our memory; nearly all our recollecting is done when optical or acoustical impressions fill the interval between learning and reproducing. The following experiments en-

deavor to submit this question to an experimental test. The time-intervals were 2, 10, 30, 60 seconds. The intervals were filled with either optical or acoustical impressions, rich with associations. The optical filling was secured by exposing printed matter, usually newspapers, vertically on the screen which covered the series, so that the page was read from the moment when the series disappeared. For acoustical disturbance the conductor read aloud newspapers, etc., with sufficient expression to interest the subject, who was sitting with closed eyes. The investigation was made in connection with the preceding one. In order to secure an accurate comparison, the three vacant intervals were immediately followed by the four filled ones, optical and acoustical filling alternating regularly.

The filling of the intervals hinders the memory. The general average of errors for the six subjects is:

	Empty interval.	Optical filling.	Acoustical filling.
2 sec.....	25.2	29.4	34.7
10 sec.....	28.8	31.0	36.0
30 sec.....	31.1	33.0	37.1

It is manifest that the acoustical disturbance weakens the memory more than the optical.

As one half of the series is visible and one half audible, the question arises: What relations exist between the kind of material and the kind of filling? Taking the total results for the four filled intervals, we have:

	Optical filling.	Acoustical filling.
Visible contents.....	34.5	33.3
Audible contents	31.4	38.3

The eye-memory is therefore more sensitive to optical disturbances, the ear-memory much more to acoustical. The most effective disturbance to recollection is homogeneous to the sense employed in perception—a fact which will show itself important for the understanding of the psychophysical mechanism of memory. For both fillings the memory shows substantially the same variations with the five contents as it does for the empty intervals. For both fillings and in each of the intervals the hinderance is least for the numbers, and increases

for colors, forms, words, and syllables; but while for all the other contents the acoustical filling hinders more than the optical, the opposite is true for the words, as the following table shows:

	Numbers.	Colors.	Forms.	Words.	Syllables.
Optical filling ...	8.0	21.6	27.1	46.6	63.3
Acoustical filling.	13.9	24.9	31.9	42.9	66.6

The special analysis of the two kinds of error shows that the relative amount of complete forgetting compared with mere misplacements increases with the filling of the intervals. Still greater is the increase of 'alien' words and syllables; these occur twice as frequently with the acoustical as with the optical fillings. The location of the errors for both fillings agrees substantially with that for empty intervals.

It is interesting that the recollection-time is largely increased by the filling of the interval, and that also here the time and the number of errors closely correspond:

	Empty.	Optical Filling.	Acoustical Filling.
2 sec....	45.4	48.6	53.9
10 sec....	47.2	54.0	57.1
30 sec....	48.8	56.9	58.0
60 sec....	—	60.2	57.7

And for the different contents:

	Numbers.	Colors.	Forms.	Words.	Syllables.
Empty	26.6	32.5	43.2	74.7	81.9
Optical filling	30.5	43.4	51.1	81.4	110.9
Acoustical filling ...	36.6	46.0	52.9	75.2	117.0

The time for words with acoustical filling is shorter than with optical filling. Even this corresponds exactly to the relations of the errors; and in the same way we find again the parallelism of the order of subjects arranged according to time or to errors. *The quicker the memory is discharged the better is the result, even when the subjective feeling of certainty is the opposite.*

Finally, we made during the whole year the same experiments with intervals of 2 and 24 hours. The experiments were done in the same stage of training as the others, but were less numerous. Each subject made only 80 experiments. A filling of the intervals with purely optical or acoustical

impressions was here of course impossible. The subjects were engaged in the ordinary duties of university students, but the 2 or 24 hours never included any other memory or association experiments. The result shows that the number of errors steadily increases with these large intervals:

	Numbers.	Colors.	Forms.	Words.	Syllables.
2 hours....	11.3	27.6	20.6	64.1	50.8
24 hours....	22.3	49.4	37.9	72.5	76.6

The character of the errors is very much changed, as forgetting is now much more common than misplacement. Forgetting surpasses misplacement by 20% for two hours (9.4% *vs.* 29.4%) and by 25.3% for twenty-four hours (13.3% *vs.* 38.8%). The location of error shows no marked deviation from the general law.

I mention, finally, that only one of the six subjects, P., was a strong visualiser. He visualized all contents. M. and N. visualized forms and numbers, R. forms only, S. forms and colors. W. did not visualize at all.

My next communication will give the results of memory experiments on the combination of form and content.

C. THE LOCALIZATION OF SOUND.

BY HUGO MÜNSTERBERG AND ARTHUR H. PIERCE.

Experiments in late years have conclusively shown that our localization of sound, in respect both to direction and distance, is much more accurate than has usually been admitted by the theories of space-sense which confined themselves mostly to sensations of touch and sight. The explanation of the phenomena, however, especially in connection with the direction of sounds, is still a matter for consideration; and this special consideration stands, of course, in the closest relation to that of the general problem of space. For the same contrasted theories are present here, and it seems not impossible that this very discussion of the phenomena peculiar to auditory space may contribute to the understanding of the general problem.

The essential factors of the mutually opposing theories of sound-localization are the following: First, the auditory sensa-

tions that come to the right and left ears are in some way different; and this original difference is the foundation upon which, by means of association, the whole localization is built up (Stumpf). Second, the sound-stimuli arouse special space-sensations in the semicircular canals. The nerves of the canals act like a sense-organ which is stimulated in various portions when the stimulus enters from different directions (Preyer). Third, the localization of the sound depends upon a judgment of the difference of the intensities received by the two ears (v. Kries, Bloch). To these possibilities a fourth is usually added, viz., that the localization is assisted by sensations of touch in the shell and drum of the ear. In opposition to these theories Münsterberg endeavored (*Beiträge*, H. II. S. 182) to develop the view that the assigning of direction to sounds rests upon the union of sensations of sound and sensations of movement, the latter originating from actual or intended movements of the head in the direction of the sounding body—a theory that carries the Lotzian notion of local signs over into the auditory field. That such movements are called forth reflexly is easily seen in children and animals. With adults, to be sure, the sound is associated so immediately with the presentation of sight that the movement itself, by which the sounding body is brought into the middle of the visual field, is from the very outset inhibited, and only the memory of previously associated sensations of movement is present.

To the theory just mentioned v. Kries, Stumpf, and others have brought forward an objection which touches at the same time the theory of accompanying sensations of touch. V. Kries says: If we localize a tone by uniting it with a sensation of movement, how then is it explicable that we are able to localize two different tones that are strictly simultaneous? If each of the two tones arouses a special sensation of movement, how can we distinguish which sensation of movement corresponds to its particular tone, since the two are not connected with each other? The result to be expected, therefore, with the ear as well as with the eye is that the two directions must be as often confused as rightly recognized. This objection does indeed offer apparent difficulties to the theory of movement- or touch-sensations; but it must not be forgotten that the

difficulty is one that belongs not merely to this individual case, but to every single phenomenon of association. When I see two colors and two names of colors arise by association in my consciousness, these two names are not coupled with the localized sensations of color, and yet no confusion occurs. This objection, therefore, points simply to a universal defect in our usual psychophysical theories. Such a theory ought, indeed, to make more allowance for the combining of groups of sensations—this, of course, is not the place to discuss it—but no one denies that such combining really happens, and its existence therefore must be presupposed for special questions. Accordingly, our lack of ability to explain this process of combination on the basis of the usual psychophysical theories ought in no way to be brought up as an argument against this individual case of association.

On the other hand, really serious objections confront the other theories. Preyer's theory, in the first place, has nowhere found approval, and rightly so; for, disregarding the purely physical objections, it is opposed, above all, by the physiological facts which show the almost certain connection between the semicircular canals and the phenomena both of dizziness and of head-movements. Psychologically also the conception of a system of auditory space-sensations is without question untenable. Still more unpsychological is the theory of localization by means of a judgment of the intensities in the two ears. This theory throws us back into the time of the doctrine of unconscious judgments, for there can be no question about the fact that a conscious comparison of the two intensities and a judgment therefrom do not exist. If we hear a sound upon the right side the coöperating of both ears may have some physiological relation to that process which accompanies the conscious localization, since the closing of the left ear would render more difficult the exact localization upon the right. But it is by no means true that two sensations enter into consciousness, one of which placed on the right appears stronger than the one placed on the left. The fact is that the sensation as a whole appears to come from the right.

Stumpf's theory, which makes the sensations originally different in the two ears, manifestly fits the facts much better.

But disregarding the fact that upon this view all the details of a more exact localization must be left to a secondary association, the phenomena occurring when several simultaneous and equal sounds are given are particularly opposed to the theory. Stumpf himself remarks, to be sure (*Tonpsychologie*, Bd. II, S. 54), that the value of the local relations between the two sides may in such cases coincide. By that, however, he has in mind only the fact that two equal tones coming from both sides are not located at all in the surrounding space, but within the head. As a matter of fact, in the majority of these cases, which have been too little noted heretofore, the sound is located in the surrounding space. Instead of two sounds coming from right and left only one is heard, with a perfectly definite localization; and this localization, which is the product of both tones, appears to consciousness exactly the same as localization which results from only one tone. It is hard to see how such a coöperation of these two original spatial relations is to be conceived. At any rate there is a sharp contrast here to the facts of sight, for there two separate but qualitatively equal lights are never taken for a single light coming from a third definitely located point.

It was for the investigation of these peculiarities that our experiments were arranged, and from this starting-point they were in a position to advance to the most various allied problems, in order to contribute new material for the resolution of these theoretical questions. Of course it cannot yet be said that the question has been brought to a final conclusion; for the investigation showed rather that the conditions are extremely complicated, and every one of the interpretations mentioned offers certain difficulties, or at least never corroborates all the possible consequences that can be deduced from any one of the various theories. First of all, therefore, emphasis should be laid upon the careful examination of the facts themselves.

The apparatus used consisted of a graduated circular metal rim 1 m in diameter, which rested horizontally upon supports, and could be adjusted to any desired height, and of arcs of the same curvature as the rim, which could be adjusted in the median, transverse, or any other desired plane. The subject sat in the centre of the horizontal circle with the ears in

its plane. The head was supported from the back by a metal ring. The sounds were given by means of telephones of equal intensity and quality. The telephones could be fixed by hooks on any place on the large rims in the direction of the radius, so that they were directed exactly towards the middle of the line which connects the two drum-membranes of the subject. The wires of all the telephones went to the same commutator and received the secondary current of a small induction-coil, which was in one of Gilman's impermeable boxes. The rustling sound of the coil could not be heard at all therefore, while the telephones reproduced it so that the two or three sounds blended perfectly into a single one. The primary current of the coil worked continuously; the secondary current, which was connected by the commutator with the telephones, had to pass an electric key controlled by the experimenter, usually A. H. Pierce. The telephones gave the sound only so long as this key was closed; for very short sounds, as in Series M., a mercury contact of a swinging pendulum was substituted for the key. The loudness of the sound could be changed by a resistance-box in the electric circuit. The experiments were done during the past two years with twelve subjects—in all many thousand experiments. For all experiments 0° means in front of the subject, 90° r at the right, 180° behind, and 90° l at the left. The subject's ears were carefully tested to discover the possible presence of abnormalities. The answer was always given with closed eyes in numbers of degrees by the subject after hearing the sound.

SERIES A. Two sounds, one on each side, symmetrically placed. Duration one second.—All localizations were at 0° or 180° . The only question is in regard to the conditions which determine the one position or the other. First, individuals differ greatly, some having a constant tendency to locate at 180° , and others having a preference for 0° . R. located all symmetrical sounds at 180° , but remarked that 5° r– 5° l was nearer the head than the rest. The same individual differs at different times, and will often place at 0° the same pair of sounds that the day before he had placed at 180° , and that with equal certainty. Secondly, there is usually a certain point at which the sound seems to shift from front to back, or *vice versa*.

Thus B. usually located at 0° all pairs of sound from 10° r- 10° l to 80° r- 80° l; hence to 150° r- 150° l the sound was located now in front and now behind, while all symmetricals back of this were placed at 180° . S. wavered continually from 10° r- 10° l to 70° r- 70° l, while back of the latter point all sounds were placed at 180° . Two other subjects located at 0° up to 80° r- 80° l, back of which they wavered, up to 160° r- 160° l. Two others wavered in the region between 60° r- 60° l and 110° r- 110° l, while all sounds in front of this region were placed at 0° and all behind it at 180° . The general deduction is that all symmetrical sounds in front of 60° r- 60° l are most likely to be placed at 0° and those back of 110° r- 110° l at 180° . The sound appears always to be at one special point—never in the head, never extended over the whole field, never on the two objective points right and left, never at 0° and 180° at the same time. The result is distinctly against the pre-supposition that the sound-sensations of the two ears are originally different.

B. One sound on each side of the horizontal arc, in all possible combinations of position from 5 to 5 degrees. Duration one second.—The limitations of the localizing power and the entire lack of constancy in the localization of the different individuals are shown everywhere throughout this series. It is highly probable that any table of usual localizations for a given pair of sounds would be valid for the individual alone from whose observations the table was compiled. The differences in the shape of ear and head, as well as the differences in the hair, beard, etc., are so marked and are such important factors in determining any particular localization of sounds within a few feet of the head that no one individual's special localization can be considered a standard for those of another. Several general statements, however, can be made.

For any given point in either of the two quadrants upon one side of the median plane a point can be found in *each* of the two quadrants on the opposite side which in combination with the first will give a localization in the median plane at 0° or 180° . For example, a sound at 45° r will give 0° or 180° not only with its symmetrical 45° l, but also with a sound in the second left quadrant. Thus, for B. 45° r gave 0° with 105° l, for M. with 115° l, for W. with 130° l, for P. with 140° ; for N. 45° r

gave 0° only with 45° l, but 180° with 130° l, and for R. with 125° l.

But we can state a more general principle, of which the foregoing is only a special case. For any given point in either of the two quadrants upon one side of the median plane a point can be found in each of the two quadrants on the opposite side which in combination with the first will give the same subjective localization. Thus B. locates 10° r- 110° l and 10° r- 70° l at 20° l; 50° r- 10° l and 50° r- 130° l at 20° r; 100° r- 50° l and 100° r- 150° l at 25° r; 120° r- 40° l and 120° r- 100° l at 40° r. Similarly with other combinations, and so without exception for all nine observers.

Very similar to this principle is the fact that different individuals, or the same individual at different times, may locate a given combination in two different quadrants. Thus B. locates 0° - 110° l at 60° l and again at 130° l; 30° r- 110° l at 40° l and 160° l, etc. Or as illustration for the individual differences: sounds at 0° - 135° r by B. 25° r, by M. 65° r, by P. 160° r; at 0° - 160° r by B. 170° r, by M. 75° r, by P. 10° r.

It is characteristic that every subject feels, at the moment of his answer, perfectly sure that the sound comes from that one point only. It is obvious that the basis of these differences lies in the fact that not only 0 and 180 , but also other points before and behind, are confused when they are sounding in a combination. To the example above, for instance, 0° - 135° r, the judgment 65° r represents rather the middle; 25° r represents the middle, if 135° r is confused with the corresponding sound from the front at 45° ; and 160° r represents the middle, if 0° is confused with 180° . Just so with 0° - 160° , 170° r results if 0° stands for 180° ; and 10° r if 20° stands for 160° .

All these experiments seem to show that the tactual sensations in any case cannot have any important influence, as they would be entirely dependent upon the objective stimulations, and neither so different under the same stimulations nor so similar under so very different stimuli. On the other hand, just this might be expected for such a subjective function as the impulse to movements, as they of course must be dependent upon a combination of all stimuli and associations.

C. Sounds symmetrical, the intensity of one gradually in-

creased by moving the telephone along a graduated radius nearer the head of the subject.—The subjective sound started at 0° or 180° , and moved to the point of greater intensity. For instance, B. located $90^\circ r-90^\circ l$ at 0° , and when the intensity of the sound at $90^\circ l$ was increased, the subjective sound moved from 0° to $90^\circ l$, *through the first left quadrant*. On the next day B. located the same combination at 180° , and the subjective sound moved from 180° to $90^\circ l$, *through the second left quadrant*. Again, B. located $45^\circ r-45^\circ l$ at 0° , and when the intensity of the sound at $45^\circ r$ was increased the subjective sound moved towards $90^\circ r$ through the *first right quadrant*, while on a later date the same combination was placed at 180° , and moved from that point to $90^\circ r$ through the *second right quadrant*. On still another date B. located this combination at 0° , and when the intensity of the sound at $45^\circ l$ was increased the subjective sound was now in the *first* and now in the *second left quadrant*, until finally it settled in the former near $70^\circ l$ and moved from there to $90^\circ l$.

D. Horizontal circle, both sounds on the same side of the median plane.—When the two sounds are in a forward quadrant the resultant may be placed in a rear quadrant on the same side, and *vice versa*. In many cases when the sounds came one from each quadrant different subjects located differently, but always so that it resulted from confusing the sound in the first with a corresponding sound in the second quadrant—10 with 170, 20 with 160, 30 with 150, etc. When both sounds were objectively in such corresponding places it became quite obvious that three localizations were in conflict: the sound appeared either in the middle or at the place of the one or of the other; for instance, $45^\circ r-135^\circ r$ were located by H. at $45^\circ r$, by P. at $100^\circ r$, and by M. at 135° .

E. Sounds at 0° and 180° .—The well-known confusion between 0° and 180° is only a special case of the confusion that we have found to exist between the front and rear quadrants. The favorite localization of this combination is at 0° ; only two subjects preferred 180° . A change of intensity does not alter the judgment; it remains at 0° , even if the 180° sound becomes stronger and stronger, or comes nearer to the head; and S. placed it nearly always at 180° , even when the 0° sound

came nearer to a fifth of the distance. No such simple formula as that weak sounds are preferably placed behind and stronger in front seemed to be supported by the results in this series. All these facts speak clearly against the influence of touch-sensations.

F. Two sounds symmetrically placed on the graduated radius at short distances from the head. Several very different intensities by means of the resistance-box in the electric circuit.—The sound is localized within the head when both sounds are very near the ears, but in front of or behind the head when a few inches distant. Both results are independent of the intensity of the sounds for all subjects. For instance, B. localized within the head the sounds from 60° r and 60° l, 4 cm from the face, even with very faint sounds, but localized 6 cm in front of the eyes when both sounds were 8 cm distant from the face even with very strong sounds. The strong sound 8 cm distant was a much stronger stimulus for the ears than the faint sound 4 cm distant, and the objective intensity was in both cases unknown to the subject. This shows how independent of the absolute intensity the localization is.

G. Three sounds.—The telephones at 0° , 90° r, and 90° l, at equal distances from the centre of the head, are heard by all observers almost without exception at 180° , while 0° alone is more often heard at 0° than at 180° . The three sounds remain at 180° even when the 0° sound comes half-way nearer to the head. Sounds at 0° – 90° – 180° are mostly located at 90° , sometimes between 90° and 180° .

H. Two sounds on a vertical arc. On the median arc more sounds were subjectively perceived in front of the transverse plane than behind.—Confusions were found similar to those noted by Preyer and v. Kries, one locating in the front quadrant what another would place in the rear quadrant.

In the transverse arc it was most characteristic that all sounds symmetrically placed one on each side of the median plane were almost always located at 180° on the *horizontal* circle, while one sound alone was usually localized in the transverse arc.

J. Only one sound given, but the attention fixed upon a point at the side, without movement of the head.—The experi-

ments were made with very well-trained observers, who made no mistake of 5° for one sound under normal conditions, with the exception, of course, of that between 0° and 180° . B. localized the sounds from 45° r, 90° r, and 135° r correctly without exception under normal conditions; but when the attention was fixed upon 90° r he localized 45° r occasionally at 60° r, 90° several times at 110° , and 135° at 160° . The points on the left side showed quite irregular misplacements when the attention was fixed upon a point at the right. Similarly P., under the same conditions as above, localized 45° once at 60° and once at 65° , 135° at 160° , etc. Were the misplacements a purely psychological illusion resulting from prefixed attention, the sound from 135° r ought to have been heard nearer to 90° , not farther away from it; and 90° itself ought never to have been misplaced. As we find that on the right side all the misplacements are in the direction of 180° , while those on the other side are irregular, it shows that the directing of the attention to 90° has the influence not of bringing the sound nearer to 90° , but of increasing the angle on the whole right side. This is easily explicable on the ground of an additional motor impulse, but not on that of a comparison of intensities.

K. One sound. Eyes blindfolded and turned to 45° r or 45° l.—The results differ for different observers. Some show only an increased uncertainty and irregular misplacements, while three observers show a regular tendency to place a sound on the same side 10° or 15° more to the rear. There is nowhere a marked tendency to bring a sound between 45° and 180° nearer to 45° . The tendency is rather towards 180° . In the case of three persons the eye-movements seem to be under an additional motor impulse, while the other observers are disturbed only by the unusual and prolonged fixation with closed eyes.

L. The muscles of one side of the body strained by voluntary effort.—The result is a marked tendency to locate the sounds on that side farther away to the rear. P., with a strain to the right, localized four fifths of the sounds on the right side 10° – 20° more to the rear. B. did the same in more than half of the cases, while a misplacement in the opposite direction happened only once.

M. The sound very short, the electric current to the telephone being closed by a swinging pendulum.—With regard to the direction the results for two or three sounds are just the same as with long sounds. It was sometimes necessary to repeat the sound before a judgment was possible. The judgments for one sound show more misplacements of 5° – 10° in both directions. There is no tendency to localize very short sounds before or behind.

N. One ear fatigued by continual stimulation, the telephone with its strongest sound being pressed to the ear-shell for several minutes.—The stimulation becomes finally extremely disagreeable, even painful. Immediately after stopping the same sound moves from the right side or from the left side along the rim, and the observer gives a signal when he hears the sound at 0° . The result is for five subjects that the fatigue has no influence at all; 0° is exactly recognized. The same result is reached when, immediately after fatiguing, one sound on each side is given. The combination is localized as under normal conditions, and when the two sounds are symmetrical is always heard at 0° or 180° , and not on the side of the unfatigued ear. This localization towards the unfatigued side occurred sometimes with the subject Mü.

In close connection with the above a further question concerned us—one indeed of an essentially physiological nature, but, like so many questions of nervous physiology, one that must be answered from psychological resources. Our experiments and the psychological analysis of them show that a conscious relation of the tones to either of the ears does not exist; that sensations of touch play no essential rôle; and that one cannot speak of a judgment of the difference of intensities in the two ears; while many things go to show that the accompanying sensations of movement are to be regarded as the psychological basis of the auditory spatial relations. The sensations of movement point to motor impulses, and it is a physiological question where these impulses are reflexly called forth. Several hypotheses are conceivable. Münsterberg's theory suggested as a side issue the possible relation between the movements and the semicircular canals. This was supported by the spatial arrangement of the canals, by

their relation to the acousticus, and above all by the physiological fact that stimulation of the canals causes movements of the head. The chief objections to this hypothesis lie in physical considerations. Another hypothetical possibility would be that the impulses to movement are set free in the brain through the action of the two-sided auditory stimulus. As erroneous as it is on the one hand to believe that a comparison of sound-intensities takes place, just so possible is it on the other hand to suppose that the difference in the physiological excitation of the two ears conditions the motor impulses with which the head reacts physiologically to the sound. At the same time it can be conceived that the stimuli of touch and pressure as well as associations influence the direction and intensity of these central impulses. Both hypotheses are compatible with Münsterberg's theory of sound-localization by means of sensations of movement. With a view to the possible settlement of these questions, experiments were arranged to investigate the localization of sound during and after movements of rotation. Since there seems now to be no doubt, especially after the investigations of James, Kreidl, and others, that the semicircular canals are the organ whose stimulation produces dizziness, it must be true that if the canals are indirectly connected with the localization of sound the latter will be changed and disturbed during dizziness. For these experiments all the apparatus formerly used was fastened to a large, horizontally rotating disk, which by means of a belt could be brought into very smooth rotation of any desired rapidity. The chair was so fastened that the subject revolved exactly about the axis of the head and trunk, while the telephones retained their position 50 cm distant from the head and could be sounded during or after the rotation as desired. The observations upon dizziness in general made incidentally during these experiments will be given in another connection. Here we confine ourselves to dizziness in relation to the localization of sound.

The revolutions were relatively very rapid in order to produce strong effects. On the average ten rotations took place in from fifteen to twenty seconds, the rapidity increasing at first and then diminishing. The rapidity of the rotation could be registered upon the drum of the kymograph. The experi-

ments were extremely disagreeable to the subjects, and often left after-effects that lasted for hours. Accordingly we were obliged to confine the investigation to the more simple questions. Rotation in the direction of the hands of a watch is designated positive, that in the opposite direction negative. The fundamental phenomena of dizziness itself corresponded, of course, to what is already known from the excellent experiments of Mach, Delage, Aubert, and others. If the eyes were kept closed during the rotation and then opened when it ceased, the body itself appeared to be at rest, while the visual field moved in the direction opposite to that of the rotation. If, on the other hand, the eyes were kept closed after the rotation stopped, the after-effect was the feeling of a lively rotation of the body itself, likewise in the direction opposite to that just given. With many of the subjects this took place in a vague way; with others the number of the reverse rotations could be counted up to eight and more. It is clear that there is an apparent contradiction between the illusory movement of the body itself when the eyes are closed and of the visual field when the eyes are open, since they both take place in the same direction. The feeling that the body is rotating stops at the moment when the eyes are opened. These facts are already known from the works of Delage and others. It is also well known that there is a tendency to turn the head called forth reflexly during the real or imagined rotation, for the purpose of compensation. Many subjects feel a marked straining in the muscles of the neck on the side in question. The interesting results with the localization of sound were somewhat as follows:

If the sound of a telephone was given for a second during the objective rotation, it was *usually misplaced in the direction opposite to the rotation*; it was rarely put at the right place, and in the limited number of our experiments was never misplaced in the direction of the rotation. E.g., negative rotation: objective sound at 45° r., subjective 135° r.; Bu., obj. 135° l., subj. 90° l. Positive rotation: P., obj. 135° r., subj. at first 70° r., this changing slowly to 110° r., etc.

If the sound of the telephone was given immediately after the cessation of the objective rotation, the eyes being still

closed, it was for the most part *widely misplaced in the direction of the rotation just given*. With this question the greater part of our experiments were especially concerned. The result was that in 82% of all the cases the misplacement took place in the direction of the preceding rotation; 14% were correctly localized, and 4% were misplaced in the direction opposite to the rotation. The latter, however, were also cases which can be looked upon as misplacements in the direction of the rotation, if the well-known confusion between front and behind is considered. The following examples may serve here: After positive rotation—P., obj. 90° r., subj. 125° r.; obj. 100° r., subj. 140° r.; obj. 45° r., subj. 60° r. moving to 80° r.; obj. 125° l., subj. 90° l., etc., in short positive misplacements throughout: then obj. 180° , subj. 45° r. moving to 25° r., which is evidently no negative misplacement, but rests rather upon the confusion between 180° and 0° . Bi., obj. 110° r., subj. 160° r.; obj. 90° r., subj. 150° r.; obj. 90° l., subj. 20° l., etc. With negative rotation the misplacement was exactly the reverse. P., obj. 90° l., subj. 135° l.; B., obj. 30° r., subj. 0, etc. The experiments upon eight persons agree perfectly in this respect. *The misplacements result therefore, during and after the rotation, in the direction of the compensatory impulses to movements of the head*, which in reality indeed produce a strain of the muscles, but no actual movement (since the head is supported from behind), and which therefore do not alter the relation of the ears to the surrounding body.

It seems that these results indicate above all that the localization of auditory sensations rests upon sensations of movement and not upon the comparison of auditory intensities. The relation of the intensities immediately after the rotation is exactly the same as under normal conditions, and yet an almost constant misplacement takes place in the direction opposite to that of the subjective illusory movement, and that too from 30° to 50° —a degree of misplacement that could not possibly be conditioned by the purely auditory factors. Further, it is not a general feeling of dizziness that renders orientation difficult; for otherwise the localization would not be so definite, and above all would not be in a constant direction. Still further, it does not rest upon an imperfect orienta-

tion of the head to the body, since the position of the sound is not designated by the finger, but is given in terms of degrees. If therefore the judgment, '45° r.', is associated with a definite ratio of sound for the two ears, why should the same ratio be judged during the positive rotation as, perhaps, 20° r. and after the rotation as 80° r.? If, however, we suppose that the localization rests upon sensations of movement, it is at once clear that an increase of the impulse by means of the compensatory reflexes produces the illusion of a constant misplacement.

The localization is independent of the misplacements in the visual field, which as is known originate from nystagmus of the eyes, and which have been widely studied since Purkinje. If the telephone was concealed by a screen, so that its position was not seen, and the eyes were then opened after a positive rotation, the visual field made a negative rotation, while the sounding body did not appear to shift at all. *The fixedness of the auditory localization can indeed influence the optical impression*. When the sounding telephone was visible it seemed to the subject several times that the whole room was turning, but the telephone itself remained alone unmoved. If after the rotation, but while the eyes were still closed, the sound was given continuously for a time, it seemed to make the illusory movement too; it remained, that is, in constant orientation with the body. E.g., Mü. after fourteen rotations had the feeling of six reverse rotations, during which the sound followed continuously like a buzzing fly. It is indifferent whether the person himself or the visual field seems to turn: the sound is localized in every case in reference to the head, and that too with a displacement dependent upon the direction of the compensating strain.

As strongly as these experiments support the genetic view of localization, just as little, finally, do they say about the physiological apparatus which calls forth the impulses to movement under the influence of the sound. Had we found no influence at all from the rotation, or had we found a complete lack of orientation, the result would have had but one meaning. The above results, however, admit of several interpretations. The compensatory movements of the head are

without question given by the canals, but whether the additional impulse from the sound has the same or a different source is not shown. The stronger impulse to movement can originate from the stronger stimulation of the semicircular canals, or it may be that the canal stimulation given by the rotation receives an added impulse from another possible source—viz., from the dissimilarity in the stimulation of the two ears. In either case this remains finally a physiological issue which has no essential interest for the psychologist. The psychological question can be only this: Does the localization of auditory impressions consist in the addition to them of sensations of movements, or not? And the positive answer to this question has received a new and unexpected support from the experiments made with the rotating chair.

D. ASSOCIATION. (I.)

BY MARY WHITON CALKINS.

The investigation, an account of whose beginning is given here, was undertaken as an attempt to answer experimentally the question of the relative significance of *frequency*, *vividness*, *recency*, and *earliness* as conditions of association. The experiments were carried out in 1892-93 and in 1893-94 in the Harvard Psychological Laboratory. The subjects were ten regular students of the laboratory, with an average of 80 experiments each; besides this, the same experiments were repeated with 25 members of the writer's Wellesley College class in experimental psychology with an average of 16 experiments each. In these 1200 experiments, here discussed, is not included the long series of preliminary experiments, which was made with a view to finding the best methods, and especially to give the subjects a fair degree of practice and training in this special work. None of the subjects knew during the experiments anything about their points and purposes; they gave their attention therefore equally to every part of the experiment. It is obvious that a previous knowledge of the purpose of the experiment on the part of the subjects would seriously interfere with the value of its results. The whole question is surely one of those for which the statistical

method surpasses the method of individual analysis. The calculation has to be based, therefore, on a combination of all experiments, and as those of the Harvard students generally coincide with those of the Wellesley students, the following tables will combine both.

The experiments here reported represent only one of the two main types which we selected. The one type was acoustical, the other optical. In the acoustical experiments the associated elements were nonsense-syllables and numerals, both pronounced; the optical experiments employed colors and numerals, both shown to the subject. The twelve hundred experiments which I discuss at present *were all of the optical type*. Their method was briefly as follows: The subject sat before a white screen large enough to shield the conductor of the experiment. Through an opening, 10 cm square, a color was shown for four seconds, followed immediately by a numeral, usually black on a white ground, for the same time. After a pause of about eight seconds, during which the subject looked steadily at the white background, another color was shown, succeeded at once by a second numeral, each exposed for four seconds. The pause of eight seconds followed, and the series of 7, 10, or 12 pairs of quickly succeeding color and numeral was continued in the same way. At the close the subject at once saw a series of the colors, but in altered order, and was asked, as each color appeared, to write down the suggested number, if any such occurred. The time was kept by following the ticks of a watch suspended close to the experimenter's ear. Color and numeral were placed together in their position behind the opening of the screen, the numeral at first concealed by the color, which was then slipped out. There was thus a merely momentary pause between color and numeral. During the eight-second pauses the opening was filled by a white ground, $\frac{1}{2}$ cm behind the screen. The subject thus did not see anything in the opening except this white ground or the color, which filled the whole square or the large printed number of two digits; the moving fingers, etc., could not be seen at all. The whole series, of course, was always carefully prepared, and placed in order beforehand.

Each series was arranged to present some one color once

only, neither at the very beginning nor at the very end of the series in connection with any numeral, and to present this same color also in some emphasized combination. Such combinations were either of frequency (in this case the color was two or three times repeated with another numeral) or of recency (in this case the color occurred at the end of the series) or of vividness (here three methods were used, which will be described later). I copy actual descriptions of series of the three main varieties:

Series 89. Frequency 3:12.

Position: First series, frequent 5, 7, 9; normal 3. Second series, 4.

I. Medium gray, 29; blue, 82; violet, 61 (n); red, 23; violet, 12 (f); peacock, 79; violet, 12 (f); strawberry, 47; violet, 12 (f); light brown, 53; dark gray, 34; light green, 72.

II. Peacock, red, green, violet, medium gray, brown, strawberry, dark gray, blue.

Series 38. Recency.

Position: First series, recent: last, normal 2. Second series, 2.

I. Light violet, 25; light gray, 14 (n); medium green, 47; brown, 73; red, 28; light brown, 54; light gray, 32 (r).

II. Brown, gray, green, violet, red, light brown.

Series 178. Vividness.

Position: First series, vivid 3, normal 7. Second series, 3.

I. Dark red, 24; dark green, 40; blue, 783 (v); orange, 57; light peacock, 15; brown, 82; blue, 61 (n); gray, 29; strawberry, 78; dark violet, 36.

II. Peacock, red, blue, violet, orange, brown, gray, green, strawberry.

To such a series the definite question of the experiment is of course the following: In what proportion of cases will the accentuated color, e.g. violet (as in series 89, just quoted), suggest the numeral—here 12—with which it is repeatedly (or vividly or recently) combined instead of the other numeral—here 61—with which also it appeared. This comparison of the suggestiveness of a color in frequent, in recent, or in vivid combination with its power to suggest when it is only once and unemphatically connected with a numeral, shows the value

of frequency, of recency, and of vividness as factors of association; it is at the same time preparatory to our ultimate purpose—the determination of the comparative value of these conditions of association.

To gain a basis of comparison about six hundred of the series have been studied as a mere memory test, leaving out of account for the time being the frequently, recently, or vividly combined numerals which they contain. Roughly speaking, about one fourth of the ordinary combinations in the longer series (10 to 12 pairs) and one third in the shorter series (7 pairs) are remembered.

TABLE I. CORRECT ASSOCIATIONS.

	Number of Series.	Possible Correct Associations.	Actual Correct Associations. Full.	Half.	%
Long series (freq., viv.)....	591	5291	1190	401	26.3
Short series (rec.).....	175	700	210	68	34.8

The tabulated results of the experiments on frequency as a condition of association are as follows:

TABLE II. FREQUENCY.

Number of Series.	Both.			Normal Alone.			Frequent Alone.		
	Full.	Half.	%	Full.	Half.	%	Full.	Half.	%
Freq. 3 : 12.... 216	32	4	15.7	9	15	7.6	100	6	47.7
Freq. 2 : 12.... 143	16	7	13.6	8	16	11.2	29	3	21.3

The table shows the number of those cases in which both numerals were recalled, then the number of cases in which the color suggested only the numeral with which it has been but once associated, and in the last group the number of times in which the color suggested only the numeral with which it had been three times or twice combined. Under the heading 'Half' are given those cases in which one digit of the numeral was recalled, and in estimating per cents these cases are rated as half correct. The comparison of the frequent with the normal shows that with a frequency of one fourth the frequently-associated numeral is recalled 63.4% ($47.7 + 15.7$), the normal one only 23.3% ($7.6 + 15.7$), and the frequent is recalled more than six times as often as the once-associated numeral in those cases in which one numeral only is suggested. The comparison of

both these per cents with that representing the likelihood of recall for such long series (Table I) leads to the same result. The frequently-associated numeral is remembered more than twice as often (63.4% instead of 26.3%), while the other numeral is remembered less than the average (23.3% instead of 26.3%). This latter result shows the negative result of habit, since the effect of habitual association with a given stimulus is seen to be a small decrease of the likelihood of ordinary association with the same stimulus.

It seems very remarkable how much this influence of frequency is lowered when the frequent association is only twice repeated instead of three times. The second line of Table II gives the results. While the negative influence of the habitual association on the once-associated numeral becomes a little smaller (it is recalled 24.8% instead of 23.3%), the positive influence of the repetition decreases rapidly; the frequent numeral is recalled 34.9% ($21.3 + 13.6$); that is, only 8.6% more than the average of ordinary associations without repetition, and 28.5% less than the three times repeated associations.

In the attempt to fix a rate of associative recency series were used varying in length from 4 to 7 pairs; the color in question was always last in the first series and second in the following series. Only the series of 7 pairs proved suitable to the purpose, for in the shorter ones both numerals were usually remembered, so that the comparison became impossible.

TABLE III. RECENCY.

Number of Series.	Both.			Normal Alone.			Recent Alone.		
	Full.	Half.	%	Full.	Half.	%	Full.	Half.	%
54	5	2	11.1	5	2	11.1	21	4	42.6

The last of the series is thus recalled 53.7% ($42.6 + 11.1$), the other numeral associated with the same color only 22.2%. We saw that the average for series of 7 pairs was 34.8% (Table I). The likelihood of recall increases, therefore, by the recency of the position 18.9%, while the negative influence on the second associated numeral is 12.6%.

The next table summarizes the records of all the experiments in vividness, separating the results according to the

different devices used to make the combinations vivid. Since the color remained the same the result could be gained only by varying the numeral, which was accordingly either black of two digits but much smaller than the other numerals, or black of usual size but of three digits, or of usual size and of two digits, but red or of usual size, but of three digits and red.

TABLE IV. VIVIDNESS.

Nature of Vividness.	Number of Series.	Both.			Normal Alone.			Vivid Alone.		
		Full.	Half.	%	Full.	Half.	%	Full.	Half.	%
Black, 3 digits.....	147	9	6	5.2	11	2	8.2	63	4	44.2
Black, 2 small digits.	102	7	2	7.8	12	6	14.7	21	3	22.1
Red, 3 digits.....	132	11	5	14.7	11	0	8.3	39	18	36.3
Red, 2 digits.....	159	18	6	13.2	12	4	8.8	53	21	39.9
Total vivid.....	540	51	19	11.2	46	12	9.6	176	46	36.8

Regarding the totals only we find that 48% (36.8 + 11.2) of the vividly associated numerals are recalled and 20.8% of the normal associations of the same colors. The general average for series of this length was 26.3%. The vivid, like the habitual and the recent association, obliterates therefore the ordinary association. On the other hand, the increasing influence of the vividness (48%) is by far not so strong as that of frequency with three times repetition (63.4%), but stronger than two repetitions (34.9%). The comparison of the different forms of vividness shows an interesting preponderance of associating three-place black numerals (52.4%) over associating two-place black numerals (29.9%). Since the latter, by reason of their quite unaccustomed small size, were decidedly impressive visually, it seems not unlikely that this difference is due to the fact that the numerals of three digits introducing as pronounced the word 'hundred' offer important aid to articulatory memory whose method is the repetition of the word. The effect of vividness is also shown by the relatively great number of cases in which numerals associated with bright colors are remembered compared with those in which the color was dark or indifferent.

A fourth kind of prominence can be given by the earliness of the association, especially by its position at the very beginning of a series; the term 'primacy' may be used. We found for all series of 12 pairs an average recollection of 26.3%, for the three-times repeated associations 63.4%, for the vivid associations 48%, for the twice repeated 34.9%. The total result for the

influence of primacy is 33.6%—that is, 7.3% more than the un-emphasized association. These 33.6% of primacy-associations are the result for those series in which the color of this first pair was given in no other combination; therefore no negative influence of the competition with frequent or vivid associations existed. This competition, on the other hand, is brought out in the following experiments. At first a direct comparison of this factor of primacy with that of frequency was made by showing shorter series, in which the same color appeared with one numeral in the first place of a series, and was then twice repeated with another numeral.

TABLE VI. FREQUENCY AND PRIMACY.

Number of Series.	Both.			Primacy.			Frequency.		
	Full.	Half.	%	Full.	Half.	%	Full.	Half.	%
60	13	2	23.3	3	2	6.6	32	1	54.2

The frequent is recalled 77.5%, the early only 29.9%. The record leads to the significant pedagogical conclusion that early associations, in spite of their tenacity, may be replaced by later ones if they are sufficiently repeated.

The influence of recency, too, can be studied in those series which were arranged without this immediate end in view; that is, in the frequent and vivid series. The recent—that is, the last of the series—is here again in no competition with others; the general result for all these series is here only 22.9% for recency, still less than the average. This decrease is evidently the result of fatigue; the twelfth pair is not observed with the same attention as the earlier ones, as we saw that the last pair in a series of seven pairs was recalled 53.7%. In any case those 22.9% indicate clearly the relative unimportance of the recent connection. Direct comparisons of recency with vividness and with frequency tend to the same result; they show an undoubted preponderance of the vivid or frequent numeral over the recent. The series was composed of seven pairs only.

TABLE VII. RECENCY AND VIVIDNESS.

Number of Series.	Both.			Recent.			Vivid.		
	Full.	Half.	%	Full.	Half.	%	Full.	Half.	%
60	13	4	25	6	2	11.7	13	10	30.0

The vivid numeral is recalled 55%, the recent only 36.7.

TABLE VIII. RECENCY AND FREQUENCY.

Number of Series.	Both.			Recent.			Frequent.		
	Full.	Half.	%	Full.	Half.	%	Full.	Half.	%
62	19	6	35.5	7	1	12.1	14	6	27.4

The twice-repeated numeral is recalled 62.9%, the recent one only 47.6%.

These last experiments must be continued in order to present a broader basis for conclusions. All the experiments together suggest, however, the hopeful probability that vivid or multiplied lines of association may be established in the individual consciousness, firm enough to withstand the force of the recent and the accidental, and powerful enough to counteract the pressing influences of the environment.

ÆSTHETICS OF SIMPLE FORMS.

(1) SYMMETRY.

BY EDGAR PIERCE.

The experiments I am about to describe were carried on during the last two winters in the Harvard Psychological Laboratory. They in some way form a connected series. So I shall begin with the more simple and continue later with the more complicated. But first of all I must describe the instrument used.

The aim of this instrument is to furnish a uniform black surface, on which various forms or lines can be moved by some device which shall not disturb the plain black background desired. Moreover, means for recording the position of the lines are necessary. To meet these requirements we have a surface of hard rubber about 1 m square; this surface is covered with black cloth. Two slits are cut straight across the board from side to side; these slits are 5 cm apart, and are so narrow and so carefully finished with cloth and black velvet on the back that no light can pass through; yet they are wide enough to allow a thin piece of tin to be inserted. Now suppose you take a piece of tin 10 cm long and 1 cm wide, and bend at right angles but in the same direction sections of this strip 2.5 cm from each end. These bent ends can now be put

into the two slits and the effect will be that of a line of tin 5 cm long resting on the black surface. Now if the two ends be so arranged that they can be attached to a slide moving on a track on the back of our board, and if the track be divided into millimetres, we then have the means of moving our line along the board from one side to the other and of recording the exact position at any moment. Of course any form may be attached to our piece of tin, and the number of slides may be increased to as large a number as desired,—we used six slides,—thus furnishing the means for an unlimited number of complicated arrangements of forms or lines in one plane along one line. Moreover, the whole instrument is fixed on a stand in such a way that the whole board can be turned in any direction. So that the slit across the centre can be made to run horizontally, vertically, or obliquely, at any angle. By an arrangement of pulleys on the side of the board it is possible for one to sit at a distance from the board in front of it and by small strings to move the lines as one pleases. The instrument stands in the dark room, which is painted black throughout; an artificial light placed at the side of the subject furnishes illumination; the eyes of the subject are protected by a black screen between him and the light.

At the present time I wish to treat the experiments from one point of view only—in respect to the æsthetical feeling of symmetry. More especially I wish to study the effect of contents of an unlike nature appearing on either side of some point regarded as a centre; for instance, the same form on both sides but different colors, or lines of the same length but not of the same breadth, and especially how far differences in content may be compensated for by other variations; for example, differences in length of lines by distance from the centre, difference in colors by variations in length, etc. The question then is: Can a feeling of symmetry, that is, of æsthetical equality of the two halves, remain when the two sides are not geometrically identical, and if so, what are the conditions under which this can result—what variations of one side seem æsthetically equal to the variations of the other side? It is clear that our instrument is fitted for an experimental study of these questions. Take the simplest case. We have in the middle of our

black field one large white vertical line 20 cm long; on the right of it is a white line of 10 cm at a distance of 20 cm; on the left is a movable line of only 5 cm. If now the movable line be placed at the same distance from the centre as the fixed line,—that is, 20 cm away,—our feeling of symmetry is not satisfied, because the lines on each side are of different lengths; but if the movable line be pushed farther from the centre a point is finally reached where the arrangement pleases, just as if the greater distance of the short line were a substitute for the greater length of the other line. Our feeling of symmetry is now satisfied, although the figure is divided into parts of very different length.

An experiment of this sort seems to be open to an objection. It may be said that the final result pleases not because the parts are æsthetically equal, but because they are in a certain pleasing proportion one to the other, but not that of equality. I refer to the well-known doctrine of the golden proportion, which rightly holds that the division of a whole into two unequal parts according to a special proportion is very pleasing. It may then be said that the impression that our figure pleased through the feeling of æsthetical equality is illusory. Indeed, the very careful experiments of Dr. Witmer have shown what great influence those proportions which roughly correspond to the golden section have on our æsthetic feelings, and how strongly they compete in the division of a horizontal line with the symmetrical division. Our instruments readily furnish the opportunity for corroborating this. If we took three vertical white lines 10 cm long by 5 cm wide, fixed two of these lines 60 cm apart and had the middle line movable, every one of six subjects chose as most agreeable a position for the third line roughly corresponding to the golden section: here two equal halves appear too monotonous, and something must be done to give variety. The golden proportion seems to be this, and yet to give some unity as well.

In view of these facts our first question must be under what conditions this pleasure in the golden proportion is in competition with that undeniable pleasure we get from equality and repetition. The following series of experiments was therefore undertaken with six subjects. I used not only three, but

four, five, six, seven, and even eight such vertical lines 10 cm long: the question was to divide a distance which was one half, one third, one fourth, one fifth, or two thirds or three fourths or two fifths of a larger distance, the other parts being represented by one or more lines. For instance, the lines were fixed 0, 20, and 60 cm, respectively; the movable line must divide the space of 40 cm between 20 and 60 in the most agreeable way; or four lines fixed at 0, 15, 30, 60, respectively; a fifth movable between 30 and 60; or five fixed at 0, 12, 24, 36, 60, and a sixth movable between 36 and 60; or three fixed at 0, 15, 60, the fourth between 15 and 60; etc. The part to be divided was alternately on the right and on the left; the movable line in this series moved in half of the experiments slowly from the right, in half from the left. The subject, who sat at 4 m from the field, the eyes at the height of the lines, was asked to stop the movable line when it reached the most agreeable position. It will be seen that the movable line in all cases divided a space into two parts; this space, however, was a part of a more complicated figure which suggested symmetry. The question was to what extent the pleasure in the golden proportion which controlled the division of the simple space would enter into competition with the pleasure in symmetry. I confine myself here to a mere outline of the general results, as these experiments were only preparatory. The results for all six subjects for the right and left position agreed in the general tendency. If there are more than three lines the tendency to a symmetrical arrangement quickly increases. The preference for the golden proportion is finally given up. Sometimes exceptional cases occur where quite irregular forms are chosen; this is due either to a conscious association, or to the desire to break up the monotony of the figure, but not to the preference for any proportion such as the golden section. The tendency to symmetrical arrangement is strong with four lines. Still more so with five. With six or more lines the tendency changes again; the pleasure in symmetry decreases and the demand for variety increases. The explanation of these results seems clear: the pleasure in these simple forms is due to intellectual enjoyment of unity and variety. Variety increases with the number of the parts,

and with four or five lines is sufficient if all the lines are placed symmetrically, especially as the attention fluctuates in apperceiving the one or the other of these parts. For two parts the lack of variety is annoying, and suggests that unequal division of the golden section which gives variety, but which yet seems to give sufficient unity, for the two parts are still regarded as making one whole; three or more unsymmetrical parts, however, lose their unity. When we come to more than six lines we find the same conditions as with these: there is unity but no variety, for the parts are so near to each other that the object appears like a fence; no parts are discriminated, and the result is a demand for some irregularity, while the number of symmetrical parts is sufficient to give unity. The general result then is: the principle of unity is the more important and, in fact, is the only one where the parts give variety; the pleasure in the golden section as giving variety is apparent only where the lines are so few or so many as to give monotony, but even in these cases unity is easily apperceived. Thus in our future experiments if we offer variety of form and content by lines of different colors, length, and breadth, and if any æsthetical pleasure results, it must be the pleasure of unity, not analogous to that obtained from the golden section, which is more essentially variety. Where the content is so varied, only an arrangement which gives unity will be pleasing; and when the figure consists of two halves, the pleasure must be a feeling of æsthetical symmetry.

I may mention in passing that I made the same experiments in a vertical position; the results were very different from the horizontal: associations seemed to overwhelm the elementary æsthetic principles. The bottom was always of a different value from the top, and symmetry evidently played a very subordinate part. The principle here seems to be that of stability; the distances between the lines here suggest the idea of masses, and the effect must be stable, and not seem as if it were going to topple over.

We now come finally to the chief question, whether or not there is any substitution for form, color, size, etc., which will satisfy the feeling of æsthetical symmetry, and how far this sub-

stitution, if there is any, suggests an explanation of our sense of symmetry. The first experiments were done with very simple material. A white line 20 cm long was fixed in the middle; on one side a white line 10 cm long, 1.5 cm wide, 8 cm from the centre; on the other a movable line 1.5 cm wide, but only 5 cm long. At what distance will the movable line be placed in order that a feeling of balance, of æsthetical symmetry, may result? The general average for all the subjects is 24.2 cm, the minimum 15.9, the maximum 29.1. The movable line proceeded alternately toward and from the central line. The question asked the subject was, When do you like the movable line best? After they had decided they were then asked if the figure gave any feeling of symmetry or balance. With the few exceptions in which associations influenced the judgment, the answer was always in the affirmative for all the experiments.

In the next group, still with white lines only and with the same central line of 20 cm, there were two lines on each side—on one two lines of 10 cm each, on the other of 5 cm. The 10-cm lines were 15 and 20 cm from the central line. One of the 5-cm lines was also 15 cm from the centre; the other 5-cm line was alone movable. The preferred average position was 33.7 cm, minimum 29.7, maximum 38.0. It is obvious that here a greater distance is the æsthetical substitute for length of line.

There is not such a great uniformity in the results when the lines are of the same length but of different area. For example, we had on both sides lines of the same length, but on one side 1.5 cm wide, on the other 0.5 cm wide. Some of the subjects are inclined also here to substitute greater distance for greater area; others seem to abstract from the difference in area.

As these introductory experiments proved that our method of experimenting is valid, and that the answers to the questions were given with subjective certainty by the subjects, we went on to more complicated experiments, in which not only different forms but also different colors were introduced. In describing these experiments a more detailed account must be given than was necessary in the first series. Seventeen groups of

experiments with different forms were made, and in each case the form appeared in six different colors. Five subjects made the whole series—Messrs. Buck, MacDougal, Rogers, and Shipp, and the writer.

The constant centre in all these experiments consisted of three vertical lines arranged symmetrically; in all cases the central line was white and 1.5 cm wide; in groups I–XIII it was 30 cm long, in groups XIV–XVII 5 cm long. In all the groups there were also two blue lines each 12 cm distant from the middle line; they were 10 cm long, 0.5 cm wide. This large centre suggested of course much more strongly than the white line alone a comparison of the two halves from the point of view of æsthetic symmetry. Beyond these constant lines there was always on the one side a fixed line, on the other a movable figure or line of some sort. In groups I–IX the fixed line was 10 cm long, 1.5 wide, 12 cm beyond the blue line. This fixed line was in half the cases dark blue, in half light red; this was done to counteract the effect of color. The movable part on the other side was variable, the following forms being used: a line 10 cm long, 1.5 cm wide; a line 10 cm long, 0.5 cm wide; a line 5 cm long, 1.5 cm wide; two lines 10 cm long, 1.5 cm wide, 5 cm apart, moving together; two lines 5 cm long, 1.5 cm wide; a square with sides of 5 cm, a star of 5 cm diameter; a square frame with sides 1 cm wide, 5 cm long; a square on end, with diagonals vertical and horizontal. In groups X–XIII the same arrangements of constant lines was used, with the addition of five lines of different colors and sizes in the space of 12 cm between the blue line and the outer constant line; the movable parts on the other side were again a 10 cm line, a square, a star, or a square frame of the same dimensions as in the first series. In groups XIV–XIX the arrangement is the same as for the first group, that is to say, without the filling of the interval, with the one exception that the central line was 5 cm long, not 30 cm. The movable parts were again a line 10 cm long, or two lines 10 cm, a square, or a star. In each of the 19 groups the lines, squares, stars, or frames appeared in six colors—white, blue, red, orange, maroon, and green.

At first we shall examine the results from the point of view of the influence of form only, leaving the question of color

until later. In the first group every number is the average of 36 experiments for each subject; in half of them the constant line on the right is blue, in half red; the movable line is given six times, once in each of the above-named six colors. In the other groups most of the numbers are averages of only 12 or 18 experiments, but here also the forms were given in the six colors. The experiments covered the whole winter 1893-94, so that each subject repeated all the experiments several times after a considerable interval.

The numbers give the average of the distance of the movable form from the left blue line. With squares, stars, double lines, the number means the distance of the middle point.

I-IX. Centre line 30 cm. On the right constant line 10 cm long, 1.5 wide, 12 cm distant. On the left movable:

I. One line 10 cm long, 1.5 cm wide. P. 18.3; R. 13.7; M. 16.9; S. 18.0; B. 15.8.

II. One line 5 cm long, 1.5 cm wide. P. 19.7; R. 14.1; M. 20.1; S. 20.4; B. 16.1. It is obvious that all five subjects place the shorter line farther away than the longer.

III. One line 10 cm long, 0.5 cm wide. P. 18.6; R. 13.9; M. 19.7; S. 19.8; B. 16.4. Also here the line is a little farther out than in I. The line here is thinner, not shorter, but the substitution still occurs.

IV. Square of 5 cm. P. 17.0; R. 14.0; M. 14.6; S. 15.8; B. 13.2. If we compare IV with II, a square of 5 cm with a vertical line of 5 cm, we find the square nearer to the centre by an average of 3.0 cm, and as this is the middle point of the square its inner edge is nearer to the centre by an average of 5.5 cm. The star, square frame, and square on end are farther out than the square, as the following figures show:

V. Star. P. 18.5; R. 14.1; M. 17.4; S. 16.6; B. 16.6. On the average 1.7 cm farther out than the square.

VI. Square frame of 5 cm. P. 17.4; R. 14.4; M. 16.1; S. 16.1; B. 14.2.

VII. A square on end. P. 17.4; R. 15.1. M. 14.8; S. 16.1; B. 14.7. The square frame is only very little farther out than the solid square.

VIII. Two lines each 5 cm long, 1.5 wide, 2 cm apart, so that their outer edges again form a square of 5 cm. P. 19.2;

R. 16.9; M. 24.1; S. 17.1; B. 14.5. It is to be noted how much farther this square made by the two lines is placed from the centre than the solid square, the average difference being 3.4 cm.

IX. Two lines each 10 cm long, 1.5 cm wide. P. 17.9; R. 16.8; M. 17.9; S. 17.4; B. 14.4.

For the four following groups the right side had the same 10-cm line as in I-IX, but the interval between the right-hand blue line and the constant line was filled with five lines of different sizes and colors. The movable piece on the left where the interval was not filled varied as follows:

X. A line 10 cm long, 1.5 wide. P. 19.4; R. 14.7; M. 18.9; S. 21.8; B. 15.5.

XI. Square 5 cm. P. 17.7; R. 15.0; M. 16.8; S. 17.4; B. 14.8.

XII. Star 5 cm diameter. P. 18.9; R. 14.3; M. 20.6; S. 18.2; B. 15.7.

XIII. Square frame 5 cm. P. 17.8; R. 14.7; M. 17.2; S. 18.2; B. 14.8.

The four movable parts of X-XIII correspond to the movable parts of I, IV, V, VI; that is to say, a line, a square, a star, and a frame are on the left, while on the right one series has the interval filled, the other not. If we compare the averages of the two series we get the following: for

I, IV, V, VI. P. 17.8; R. 14.0; M. 16.2; S. 16.6; B. 14.9.

X, XI, XII, XIII. P. 18.4; R. 14.7; M. 18.4; S. 18.9; B. 15.2.

The distance of the left-hand form is greater in the second or filled series for all subjects; but it is to be noticed that while the difference is considerable for M. and S., for the three others it is some millimetres only.

In the four following groups the conditions of I, IV, V, and IX are given again, with the exception that the central interval here is not 30 cm but only 5 cm long. The movable parts are given as follows:

XIV. Line, 10 cm long, 1.5 wide. P. 16.5; R. 13.3; M. 19.5; S. 18.8; B. 16.2.

XV. Square 5 cm. P. 15.6; R. 13.0; M. 15.1; S. 16.5; B. 14.0.

XVI. Star. P. 16.6; R. 13.0; M. 21.3; S. 16.3; B. 16.9.

XVII. Two lines 10 cm long, 1.5 wide. P. 13.9; R. 16.7; M. 16.7; S. 18.0; B. 15.5.

Here again we find the differences resulting from the special forms similar to those of the first set: the square is nearer to the centre than the line, and the star farther out than the square; but the important point is that this group differs so much from the corresponding groups I, IV, V, IX, while the forms on both right and left remain the same, the central line only being changed. Now if we compare the averages,

I, IV, V, IX. P. 17.9; R. 14.7; M. 16.7; S. 16.9; B. 15.0;

XIV-XVII. P. 15.7; R. 14.0; M. 18.2; S. 17.4; B. 15.7,

it is evident that the variations are in both directions. M., S., and B. place the movable line farther from the centre when the central line is short; R. and the writer place it nearer under the same conditions.

In general, then, an examination of these 17 groups with regard to the influence of form tends to show that the feeling of symmetry resulting from a combination of parts not symmetrical is subject to great individual differences, as the averages of all the groups together give:

P. 17.6; R. 14.6; M. 18.1; S. 17.8; B. 15.3.

On the other hand there is such an agreement of all the subjects in regard to the relative value of the different forms, that the figures are surely not to be regarded as a matter of chance, but as the expression of constant relations. A long line must be farther out than a short one, a narrow farther than a wide; a line farther than a square, a star farther than a square; an empty interval must be larger than one filled, and so on. How are we to interpret these much-neglected elements of the æsthetic impression? Are the results to be traced to a sensational or an intellectual origin, or more especially are the sensations resulting from the muscular action of the eyes or the suggested ideas or associations the determining factor? In favor of a purely sensational explanation it seems to me are the results of groups X-XIII as compared with I, IV, V, VI. The filling of the interval had here the effect of pushing the movable part a few millimetres farther out.

If there had been an intellectual association, and if the attempt had been to balance the figure from a purely mechanical point of view, the movable form would have been placed much farther out. The small increase, however, corresponds

to the increased difficulty with which the eye moves over the filled space as compared with the empty one.

Still stronger in favor of a sensational explanation are the results of groups XIV-XVII as compared with I, IV, V, IX. The only difference here is in the central line, which is 5 cm long in the groups XIV-XVII instead of 30 cm. If the sides are precisely the same in both series there can be no reason if the comparison is intellectual in changing the relations of the movable line when the central line is changed, but we have seen that such a variation does occur for each subject. The eye-movements are, however, much changed by the change in the central line. The new eye-movements suggest new ideal lines connecting the ends of the various forms, and as every new combination allows new ideal lines, it can be understood that here the differences for each subject should become more apparent.

It is to be noted that the greatest individual differences result where there are two movable lines: here it is obvious that the subject is free to choose between the inner and outer line as the outer end for the ideal connecting lines.

On the other hand, it seems to me impossible to explain all by eye-movements alone. Two lines forming a square are regularly put farther out than a solid square of the same outlines. The association of solidity seems here to be the deciding factor. The same explanation applies where the star is farther out than the square and a narrow line farther out than a wide line.

We turn now to the question of color. It will be remembered that in the first three groups the fixed line to the right was in half the cases red, in the other half blue. The numbers above give both colors together. If we now separate them we obtain the following result:

A. Red-line groups I, II, III: P. 19.0; R. 14.2; M. 20.0; S. 20.1; B. 16.5.

B. Blue-line groups I, II, III: P. 18.2; R. 13.6; M. 17.8; S. 18.8; B. 15.7.

The movable line on the left was placed farther out when the right fixed line was red than when it was blue.

More striking are the results from the six colors used in

the movable forms: these forms appeared in each of the 17 groups equally often in each color. The averages are as follows:

P. blue 18.5, green 18.0, maroon 17.8, red 17.6, orange 17.3, white 17.0. R. blue 15.2, maroon 14.7, green 14.6, white 14.5, red 14.4, orange 13.7. M. blue 20.4, maroon 18.7, green 18.1, red 17.6, white 17.1, orange 17.0. S. blue 20.3, maroon, 18.7, green 18.3, red 17.9, orange 17.1, white 16.1.

For all these subjects, blue, maroon, and green, the dark colors are the farthest out; white, red, and orange, the bright colors are nearest the centre. This means that a dark color must be farther out than a bright one to compensate for a form on the other side. The brightness of an object is then a constant substitute for its distance in satisfying our feeling of symmetry. The order of the colors is, however, somewhat changed for Mr. Buck. B. blue 17.5, orange 16.6, maroon 15.3, white 15.1, red 15.0, green 14.7. Orange and green have here changed places. The explanation is simple: Mr. Buck is red-green color-blind.

It is remarkable that most of the men felt subjectively sure that the colors have little influence. In fact it was not until after having done many experiments on myself that I was sure the colors did have any effect, and not until I had tabulated the results could I tell in what directions they did work.

I am inclined to think that here again we have to do with the strength of eye-movements. Red, orange, and white stimulate the eye more strongly than blue, green, and maroon, and call forth stronger eye-movements by which a form with a bright color gets the importance of a larger object. This would destroy the feeling of symmetry if the forms were not placed farther in.

Our feeling of symmetry which demands unity for the two parts can then be fully satisfied by arrangements of geometrically different forms and by different colors; variations in the size of the forms and the brightness of the colors can be compensated for by variations in the distance from the centre. The general law seems to be that the feeling of symmetry is satisfied when both parts call forth eye-movements of like energy; this energy increases with the distance from the

centre or the larger size of the objects, and with the greater brightness of the color. The judgments tend to agree if a given set of eye-movements is necessary; the difference between individuals, and even of the same individuals at different times, is greater when there is a possibility of various combinations of eye-movements. All these sensational differences can be supplemented or destroyed by intellectual associations which give special parts a greater importance, as solidity, impressiveness, and so on. Greater importance works like a substitute for greater energy of the eye-movements, and a more important object must come nearer the centre to satisfy the feeling of symmetry.

THE IMAGERY OF AMERICAN STUDENTS.

BY PROFESSOR A. C. ARMSTRONG, JR.,

Wesleyan University.

(With the Assistance of Mr. C. H. JUDD.)

The data on which the following paper is based were obtained from 188 sets of answers to the well-known questions of Mr. Francis Galton on 'Visualizing and other Allied Faculties.' Of these, 37 sets came from members of the classes of 1881 and 1882 in Princeton College, and were kindly furnished to the writer by Professor H. F. Osborn, then of Princeton, now of Columbia College.* The remaining 151 sets were collected from four successive classes, 1890-93, in Wesleyan University. The answers, as a whole, represent the American student in the latter half of his college life (junior or senior year), and at a probable average age of twenty to twenty-two years. The replies were in general given after some preliminary acquaintance with the subject. The Princeton papers came from volunteers who had enjoyed the advantage of a lecture by Professor Osborn on Mr. Galton's work and a subsequent free discussion by various members of the audience. At the close of the evening 100 blanks were distributed, of which 43 were returned (6 of these being thrown out in the present inquiry as not coming from students, or for other neutral reasons). The Wesleyan papers were the answers of pupils in (prescribed) elementary psychology, and in this case nearly every member of the four classes concerned answered; in fact, many of them did it as task-work, though in the majority of cases they had already made some study of the imagination and had heard some discussion of Mr. Galton's investigations in particular.

* The Princeton answers have been discussed in part by Mr. Galton in his *Inquiries into Human Faculty* and by Professor Osborn in the *Princeton Review*.

The data may, therefore, be fairly held to represent more intelligent self-observation than can be secured from persons ignorant of psychological study altogether; and the exaggeration of personal experience, which might have been looked for under the circumstances, we have been forced by careful study to believe to have been for the most part absent. At first sight it seemed that some unconscious self-deception was indicated by such phenomena as the excess of number-forms over percentages recorded by other observers, and the remarkable number of good visualizers in the case of color reproduction; but the more the papers were considered, the stronger the conviction became that our results were fair averages for the class of persons under discussion. The Princeton volunteers gave no higher results than the Wesleyan returns. The class of '92 at Wesleyan, which appeared to have overstepped all bounds of care in their answers, were shown by a second, test set, taken from about one-half the class a year after the first series, to be entirely reliable. Cross-examination, moreover, of some of the members of another class led to the same conclusion; and in regard to at least one of the doubtful questions—No. X—there was corroborative evidence, in the shape of drawings of the number-forms which the subjects claimed to possess. Finally, it may be noted that the variations within the group run parallel to those remarked by other observers, notably by Mr. Galton himself.

The first three questions are intimately related:

"Before addressing yourself to any of the questions on the opposite page, think of some definite object—suppose it is your breakfast-table as you sat down to it this morning—and consider carefully the picture that rises before your mind's eye.

"1. *Illumination*.—Is the image dim or fairly clear? Is its brightness comparable to that of the actual scene?

"2. *Definition*.—Are all the objects pretty well defined at the same time, or is the place of sharpest definition at any one moment more contracted than it is in a real scene?

"3. *Coloring*.—Are the colors of the china, of the toast, bread-crust, mustard, meat, parsley, or whatever may have been on the table, quite distinct and natural?"

The answers to 1. may be divided into five classes. The first class consists of those in whom the faculty is present to a very high degree; the illumination here is 'as bright as the original,' or, as another member of the class puts it, 'clear, not at all blurred, as bright as the actual.' The second class yields answers such as this: 'clear, comparable to the original, but not quite so bright.' The members of the third class have images which are fairly clear, but hardly bright enough to be comparable to reality; one of them describes his imagery as "fairly distinct, but not like the original. It is as if seen through a thin mist or steam." The images of class IV are dim and not to be described as even fairly clear, while the comparison to reality is entirely dropped; and class V includes those cases in which the power is shown in its very lowest stage, or, perhaps more accurately, tends to disappear altogether. The replies from this lowest grade often indicate a lack of comprehension of the subject, with no real conception of the meaning which the questions are intended to convey. One subject says that he "feels the breakfast-table rather than sees it"; a second declares that he "never did have and does not now have any images whatever."* The following table shows the number of cases in each of these five classes, and the percentages relative to the whole number (188):

Class.....	I	II	III	IV	V
No. of Cases.....	24	91	54	14	5
Percentage.....	13	48	29	7	3

It will be noted that the large majority of cases fall within classes II and III, proving the prevalence of moderate to high degrees of capacity. The first or highest class is also well represented, but the fourth and fifth show a marked falling off.

This class-division based on 1. is in general one of the most important that can be made. Thus the answers to Mr. Galton's question IX—"What difference do you perceive

*Cf. Galton, *Inquiries into Human Faculty*, p. 84 *et seq.*, and James, *Principles of Psychology*, II. pp. 56, 57, on the typical character of these answers. The text diverges chiefly by the attempt at *statistical* classification.

between a very vivid mental picture called up in the dark and a real scene?"—show, as was to be expected *a priori*, a close connection between the degree of illumination and the sense of reality. The replies indeed are hardly susceptible of statistical presentation, but a very large proportion exhibit the relation in question. Members of the first class declare with great unanimity that they perceive no difference between the vivid image and the real scene. Those of the second and third classes usually say that the image is not so bright. Even the fifth or lowest class attribute their lack of images to the fact that all is dark; as one says, "there seems at times to be an image of a very general color, but it is too dark to be seen with any certainty."

Retaining the same fivefold division in our consideration of 2. we find that two natural subdivisions appear under each class—A, those subjects who have all the objects on the imaged table pretty well defined at the same time, and B, those whose place of sharpest definition is somewhat contracted. These subclasses are, of course, relative to the main division, for where the whole field is dim the defined portion may be called distinct only in a comparative sense. This understood, the results are these:

Class (by degree of illumination).	I	II	III	IV	V
Whole No. in each Class.....	24	91	54	14	5
A (full definition).....	19	50	21	5	0
B (definition contracted).....	5	41	33	9	5

This table shows that the extent of clear definition varies somewhat as the degree of illumination; as the latter decreases, the former contracts, with an almost continuous gradation among the several classes. Those under class I. who notice any contraction state with evident caution that "the place of sharpest definition is slightly contracted." In classes II and III most of the cases range from 'somewhat' to 'considerably contracted.' Class IV will have it 'much more contracted'; and in class V the replies indicate that such vague images as

are sometimes seen are confined to single objects or a very small point. It is worthy of notice, further, that persons who image clearly less than the real scene usually bring the fact into relation to the action of attention. As one of them says: "A point is clearly defined when my attention is turned upon it, while the objects around it are rather hazy."

The replies to 3. argue a widespread faculty of color visualization. No less than 172 of our subjects, 91% of the whole number, think that their color imagery is relatively distinct and natural, while only 16 or 9% fail in this respect. Some of the former go so far as to state that "colors are much more distinct than the general outline"; and a few in the latter division are able to bring out the missing quality by turning their attention toward it. An emphatic testimony is given by one who ranks in the lowest class under 1. when he says that if he could be persuaded to believe that he had any visual images at all, it would be because he sometimes thinks that he can (mentally) see colors.

Mr. Galton's question V presents some interesting phenomena and may best be taken up next. It runs:

"5. *Distance of Images*.—Where do mental images appear to be situated—within the head, within the eyeball, just in front of the eyes, or at a distance corresponding to reality? Can you project an image upon a piece of paper?"

Of our students 133 or 71% * state that with them the distance corresponds to reality, while 6 see their images at positions more distant than the real scene, 'as in a kind of bird's-eye view.' The remaining 49 are divided as follows: 19 localize their images in the head, 14 just in front of the eyes, 4 in the eyeballs, and 12 at variable distances. The margin of error here is probably considerable, for the 'situation of mental images' may be conceived in various ways. Thus, we are convinced, a subject may locate his images as *ideas* in the head and so reply to the question, though their apparent position in his scheme of imagery, if he could be induced to attend to this alone, would prove different. Again, the imaged break-

* This is apparently contrary to the observations of Galton, who says, p. 99, "Most persons see it [the image] in an indefinable sort of a way, others at a distance corresponding to reality."

fast-table may be thought of at its actual known distance from the subject at the moment; or he may imagine the table with himself sitting at it, and estimate distances from the standpoint of the recalled self; or he may interpret the question in the sense intended by its framer and state what position his images tend to assume apart from special influences. But these variable factors aside, there is evident in some cases a marked connection between the apparent distance of images and the effort of attention necessary for their production. Thus our students write: "Some mental images seem to be in front of the eyes. These are generally some small objects, as a photograph, inkstand, etc., or images that have cost me severe strain to recall. Images of scenes, views, etc., and all such as come without special mental strain are just behind the eyes"; "Large objects as in reality. Smaller in the head or just before the eyes"; "If a reproduction of a particular scene, at a distance corresponding to reality. If imagining some small object without regard to a previous scene it appears to be about four yards in front of me. Large objects as far distant as I can regard them with best advantage"; "Mental images appear situated at a distance corresponding to reality when recalled without intense effort." Two things are to be noted here: first, that the images of small objects tend to be situated closely in front of the eyes; and second, that when the image is recalled with difficulty a like effect is produced on the apparent localization. In regard to the former phenomenon it may be enough to refer to the fact that we habitually look at small objects from a near position, but in reference to the second we may go a little deeper. Here the influence of the effort to recall on the apparent distance must be considered in connection with the further facts that of these 49 persons with whom the position of the image is other than that of the real scene, 39 report (in answer to 2.) the point of sharpest definition in their imagery to be considerably contracted, and that such contraction, as noted above, is often a function of the strained attention necessary for clear visualization. It is impossible not to conclude that, at least in some cases, the situation of visual images depends on the attention involved; while the inference is near that this phenomenon may be connected with

the feelings of change in convergence and accommodation which so often accompany concentrated visual attention in the first instance. If the strain of recall should suggest these usual associates of concentrated visual attention, and if these in turn should exercise their wonted influence as determinants of distance, the case would be clear.*

The remaining questions are of a more special character and deal for the most part with particular modifications of the visualizing function. The fourth, which we may now take up, runs:

"4. *Extent of Field of View.*—Call up the image of some panoramic view (the walls of your room might suffice); can you force yourself to see mentally a wider range of it than could be taken in by any single glance of the eyes? Can you mentally see more than three faces of a die, or more than one hemisphere of a globe, at the same instant of time?"

To this 130 of our subjects, 69%, either return a general negative or give doubtful answers. With 58 or 31% the capacity is very variable. Thus one can see mentally 'just a little more' than the range of a single glance; another is confident that he can 'see a whole sphere'; and a third describes the image of his "whole room as if he were looking through his head." The replies of others suggest Mr. Galton's statement that "some persons have the habit of viewing objects as though they were partly transparent"; while two of our cases throw light on the manner of effecting the feat described in the question which some of his subjects term 'a kind of touch-

* A striking example of the effect of attention on the position of images was observed in the case of Mr. A. G. C., who, as he is not a student, could not be included in the statistics of the text. Mr. C. is a gifted visualizer. His images are so habitually localized as in reality that cross-examination failed to show a single exception. He is also endowed with the gift of mentally 'seeing around objects,' and can image four faces of a die without difficulty. The visualization of all six faces, however, costs him an effort of attention, and when he was asked to accomplish it, it was noted that the hand which he held in front of him to aid his visual imagination (as though the die were held in it) *moved up toward the eyes*. Questioned about the fact, Mr. C. recognized the movement, although he had performed it involuntarily, and was able to estimate it in inches since his business and habits accustom him to measurement. Without strain he holds the hand and visualizes images at about 14 inches from the eyes; with strain they move up to a distance of 4 inches.

sight.' One student says that he is in doubt as to his ability to visualize more than can be taken in at one glance, but adds in a note: "I can easily imagine putting my fingers around the six sides of a cube at the *same* time, and so take in (perceive) more than one or two or three sides of the cube." Mr. A. G. C., it will be also remembered (cf. note above), held the imaginary die in the hand when endeavoring to visualize it; so that we may surmise that with these gentlemen experiences of active touch, imagined or actually present, aid the effort of vision to grasp the whole field. A second student cannot visualize the spots on the die, but can readily recall more than three sides of a plain cube; and here it is possible that ocular movement or adjustment may account for the ease in imaging the general form in contrast to the colored spots. But we should be slow to deny that some gifted visualizers can in fact 'mentally see' around objects with little or no help from other than purely visual sources. The positiveness of their assertions on the matter go far to offset any theoretical difficulties which may be raised.

Question VI relates to command over images:

"6. *Command over Images.*—Can you retain a mental picture steadily before the eyes? When you do so, does it grow brighter or dimmer? When the act of retaining it becomes wearisome, in what part of the head or eyeball is the fatigue felt?"

The answers to the first two clauses of this question may be divided into five classes. First, 11 persons say that they cannot retain their images at all. Of the remaining 177 or 94%, the second class find no change in the illumination of the retained picture; while the third notice an increase of brightness, this being shown by their replies to depend in many cases on the focussing of attention. As one says, "By severe strain I can retain an image; it grows brighter and more distinct; the object comes out better"; and a second, "It tends to grow dimmer; but that depends on the passivity of the state: could will it to grow brighter." The fourth and much the largest class shows a decrease of illumination, the diminution varying to the extreme limit. With some in this class the image vanishes suddenly after a period of retention;

others notice that 'the images gradually fade out in thirty seconds or a minute.' The fifth class show a kind of rhythm in their imagery which is very interesting, a period of brightness being followed by a period of dimness, which in turn gives place to bright images again, etc., etc. In the words of one member of the class, 'the images are intermittent'; or as another characteristically puts it, "Images come by flashes, then die away. Flashes can be produced in quick succession and continued indefinitely without weariness." The several classes may be thus tabulated:

Class.....	I	II	III	IV	V
No. of Cases.....	11	30	43	88	16
Per cent of whole No.	6	16	23	47	8 +

The final clause of the question brought out no very definite results. In a number of cases no fatigue at all is felt; when it does occur, it is usually located in or about the eyes.

In answer to question X.

"10. *Numerals and Dates.*—Are these invariably associated in your mind with any peculiar mental imagery, whether of written or printed figures, diagrams, or colors? If so, explain fully, and say if you can account for the association?"

That they have no such associations whatever is stated by 105 persons or 56%, 67 or 36% say broadly that there is an association with the written or printed figures, and 16 or 8% that they have number-forms. The last of these classes is relatively very large. Among its members there is one interesting case in which a number-form used in childhood had fallen out of use in later life and even out of mind. When the gentleman in question began the study of French in his Freshman year, however, the old form was revived and the French names for the numerals learned in association with it. The matter thus being brought back to memory, the same subject recollected further that he had once possessed an alphabet-form as well, and that as he first learned the Greek alphabet he had made a modification in the form, apparently to fit the different order of the Greek letters. All these forms have now been

again discarded ; and the case as a whole is a good illustration of the decay of visualization as age advances and abstract thought increases.

Such, then, are the general results for the visual imagery of American college students so far as indicated by the data at our command, for the remainder of the questions developed no very new or noteworthy facts. The most striking phenomenon shown is the intimate relation of imagery and attention and the effect of the latter on the various phases and characteristics of the former. The question of the relative capacity of our students and other classes of persons is not shown by these results ; but in general we estimate that the students stand higher than the English scientists and men of letters whom Mr. Galton examined. Within the group of Wesleyan students an attempt was made to compare the power of visualization with the standard of scholarship as tested by college grades. The outcome proved the data to be inadequate for the determination of the problem, but as far as the figures go they point to a little lower average of imaging power among those who stand highest in their respective classes. It would be instructive to have a comparative report from female students of a similar age and stage of advancement. The few sets of answers which we have obtained from ladies at Wesleyan show, as might have been expected, a higher development of the faculty among them than among the men ; and a small number from Vassar College point to the same conclusion.

THE PENDULUM AS A CONTROL-INSTRUMENT FOR THE HIPPI CHRONOSCOPE.

BY LIGHTNER WITMER, PH.D.,

University of Pennsylvania.

The necessity of controlling the constant error of the Hipp chronoscope for longer times than 100σ , the maximum obtainable with the Cattell drop-screen, and the many objectionable features of the new enlarged Krille control-hammer, led me in the fall of 1892 to investigate the accuracy with which some form of pendulum could be made to record a suitable range of absolute time-periods. From an examination of the mechanical principles involved in both instruments it would seem that the constancy and accuracy of a pendulum must be at the very least as great as that of the control-hammer; for the latter is in principle a pendulum also, set free from a point above the axis of suspension and arrested in the first part of its swing.

The tests were made upon a compound pendulum of very simple construction. An iron bar 141 cm in length, in cross-section an ellipse 2×1 cm, was hung upon knife-edge bearings of steel in such manner that the axis of suspension was at a distance of 72.3 cm from the lower end. A heavy bob of iron weighing 5 kg moved up and down the lower portion of the bar and could be secured by a thumb-screw at any desired point. A lighter bob, 2 kg in weight, was similarly adjustable on the part of the bar above the axis of suspension. The knife-edge bearings restricted the amplitude of oscillation to about 60° , or 30° to each side of the vertical position. A small brass marker projected below the lower end of the pendulum and moved to and fro just above a metal scale, conforming to the arc of oscillation, and graduated in tenths of degrees from the vertical position, the zero point, to 25° in both directions. Two contact-pieces were movable upon this arc.

scale and could be set at any desired mark on the scale. These contact-pieces were self-adjusting. When set free, the pendulum swung from left to right; the projecting marker, striking the first contact and turning the disk of the contact-piece with a minimal expenditure of energy, closed an electric circuit; the pendulum moved on, the marker struck the second contact, and the circuit was broken. On the return swing of the pendulum, the marker in similar manner readjusted the contacts for the next experiment. The contact-pieces could also be so arranged that the swing of the pendulum first breaks and then closes a circuit. The accuracy and constancy of the time-records of the pendulum depend upon the accurate working of the contact-pieces. Those made for these tests were supplied the laboratory by Mr. J. D. Brown, of Camden, N. J., and were the best that I had seen up to that time. Some difficulty, however, was experienced in properly adjusting the amount of pressure just sufficient to prevent the blow of the pendulum from sending the circular disks of the contacts too far around. Because of this difficulty and on account of a subsequent injury to the readjusting mechanism, the projecting pointers of the contact-pieces, which are struck by the marker of the pendulum-bar, were put in place by hand before each experiment of the series tabulated below. When the self-adjusting mechanism worked at all, the results were as satisfactory as when the contacts were reset by hand. On a day, for example, when the chronoscope registered 100.1σ as the fall-time of the Cattell drop-screen, the average of 20 experiments, the mean variation being 0.69σ , the same chronoscope recorded a time-value of 96.4σ given by the pendulum, the contacts readjusted by the return swing, with but 0.76σ as the *mv* of 40 experiments. This mean variation is about the same as the average mean variation of the tabulated experiments. A second set of contacts, in which the difficulty in adjusting the pressure properly had been done away with, was furnished the laboratory, but was sent to Chicago before its accuracy could be tested and was there stolen. Mr. Brown has lately supplied the laboratory of Columbia College with an improved form of this recording device.

The pendulum was held in place at the extreme left of the

arc by a metal catch, and was released and replaced on its return swing by hand. More convenient is a magnet of sufficient strength to catch up the pendulum-bob on its return swing. The whole apparatus is then self-adjusting for any number of experiments after the contact-pieces have once been put in place on the arc-scale, and is controllable at a distance by an on-and-off key.

The present experiments had as their object to compare the variable error of the pendulum with that of the Cattell drop-screen. The latter instrument is used in the laboratory of the University of Pennsylvania to control the Hipp chronoscope. The screen occupies exactly 100σ in falling through the distance between the contacts, as determined by calculation and by the chronographic method. Spring and current are adjusted until the chronoscope gives a reading of 100σ as an average of 10 experiments, with a mean variation of not more than 1σ . If the mean variation exceeds 1σ , the circuit or drop-screen or chronoscope is overhauled until the variable error falls within the proper limits. The large $mv\ 2.2\sigma$ obtained by Külpe and Kirschmann* from ten records with the screen, whose average result was 63σ , was no doubt due to some imperfection in the contacts and would have been diminished with care and use (the instrument was a new one). Of six series of 20 chronoscopic records of the maximum drop, 100σ , the average mean variation as shown in Tables I and II was 0.66σ . Within the limits of the time-values given by the free drop of the screen, the instrument has given complete satisfaction as a control to the chronoscope.

The experiments of Tables I and II were made and averaged in series of twenty. No experiment was excluded from the series of Table I for any cause whatever, and averages throughout represent 20 successive experiments. But to obtain the longer times of Table II the pendulum was allowed to swing to and fro a number of times, and the experimenter on several occasions failed to arrest the pendulum at the proper moment. For this reason it was necessary to exclude four experiments while the series of 20 tests was being made.

* *Ein Apparat zur Controle zeitmessender Instrumente.* Beschrieben von O. Külpe und A. Kirschmann. Wundt, Phil. Stud., Bd. VIII. Heft 2, p. 148.

If the first experiment or occasionally the first two seemed to vary by a greater amount from the average than any of the remainder, one or two additional records were taken immediately, and if the last two varied less than the first two from the general average, the last twenty were used to obtain the tabulated average. But no results were excluded for any reason from the body of the twenty successive records after the series had been closed and the operation of calculating the average and mean variation begun.

In Table I are given the results of 280 chronoscopic records of a time-period of *circa* 100σ, given by the swing of the pendulum from contact to contact. No attempt was made to measure by the chronographic method the absolute time-period; what this actually was is to be inferred by comparison with the fall-time of the drop-screen. To obtain the results of the first series of No. 1 (*Av.* 101.0σ, *mv* 0.25σ), the upper bob was taken off, the lower bob raised as high as it would go, the pendulum consequently moving at its fastest rate. One contact—the making contact—was placed as far as possible to the left of the arc (at -24.7°), the pendulum being set free at -28° . The breaking contact was then tried in various positions until the chronoscope recorded a time-interval of approximately 100σ. When this had been determined and the position (-15.95°) noted, the twenty records were taken from which the average result of 101σ with *mv* 0.25σ was calculated. The contact-pieces, with bobs unchanged, were next tried near the centre of the arc, at the point of maximum velocity and momentum. With the contact-pieces at -7° and $+5.6^\circ$ the chronoscope recorded an average result of 99.5σ with 0.5σ *mv*. The lower bob was then moved to the bottom of the bar, reducing the rate of oscillation, and again the positions at the bend and centre of the arc were found at which the chronoscope registered *circa* 100σ (Nos. 3 and 4). By using the upper bob and raising the lower, the rate was further reduced (Nos. 5, 6, and 7). Four rates were used, which are noted in the table as 'very fast,' 'fast,' 'slow,' and 'very slow'; the corresponding time of a single oscillation was roughly estimated at just less than $\frac{1}{4}$ sec., $\frac{2}{3}$ sec., 1 sec., and 4 secs. respectively. After the seven series had

TABLE I.

CHRONOSCOPIC RECORDS OF SWING OF CONTROL-PENDULUM AND OF FALL OF CONTROL-SCREEN. AVERAGES AND MEAN VARIATION OF 280 EXPERIMENTS IN TERMS OF SIGMA. SERIES OF 20 EXPERIMENTS.

Pendulum.						Drop-screen.	
No.	Position of Bobs.	Rate of Swing.	Position of Con- tacts.	Av. Re- sult.	MV.	Av. Res.	MV.
1	Upper bob off. Lower bob raised to highest point.	Very fast.	-24.7°, -15.95° To left of arc and beginning of swing.	101.	0.25	100.1	0.92
				100.9	0.45	100.1	0.68
					0.35	100.0	0.65
2	"	"	-7°, +5.6° Centre of arc.	99.5	0.5	100.2	0.69
				98.1	0.58	100.2	0.48
					0.54		
3	Upper bob off. Lower bob at bottom of bar.	Fast.	-24.7°, -17°	99.	0.5		
				99.8	0.63		
					0.56		
4	"	"	-7°, +4.5°	100.2	0.68		
				99.6	0.74		
					0.72		
5	Upper bob at top. Lower bob at bottom.	Slow.	-24.7°, -20.3°	96.1	0.84		
				97.8	0.84		
					0.84		
6	"	"	-7°, +0.1°	102.1	0.46		
				101.0	0.35		
					0.40		
7	Upper bob at top. Lower bob raised to bal- ance upper.	Very slow.	Centre of arc. Very close to- gether. No record in de- grees.	100.5	0.6		
				100.	0.55		
					0.57		
				Av. Mv.	0.57	Av. Mv.	0.68

been completed for the seven different positions of bobs and contacts, they were repeated by placing the contacts without preliminary trials at the marks found in making the first series. The average variation of each series of two aver-

age results from the average of the two series is a measure of the accuracy with which the contacts may be set to a mark to record a desired time-period. This average variation is not given in the table, as no particular attention was paid at the time to the accurate replacing of the contacts. But this average variation for all results is only 0.44σ , the maximum being 0.85σ (No. 5).

The average results of the 14 series of 20 tests with their *mv* are given in vertical columns, together with the positions of bobs and contacts and with the rates of speed. Before, during, and after these tests, 5 series of 20 chronoscopic records each were taken of the fall-time of the drop-screen, to see that the constant error of the chronoscope had been reduced to zero and remained so, and to obtain data for comparing the variable error of the pendulum with that of the drop-screen. The results of these 100 records are given in vertical columns, next to the results of the pendulum series. The general averages at the bottom of the table show the average mean variation of a record with chronoscope and drop-screen in 100 experiments to be 0.68σ ; that with the chronoscope and pendulum in 280 experiments to be 0.57σ . From this we conclude that for the time of 1000 the pendulum is as satisfactory a control-instrument as the drop-screen.

A comparison of the mean variations among themselves, for different positions of bobs and contacts, shows that with the pendulum swinging 'fast' and 'very fast' the best results were obtained at the beginning of the swing; but with 'slow' the centre of the arc is better. A medium rate would seem to give the best results. Too severe a blow upon the contacts or a too slow movement of the disks is relatively the more unfavorable condition. But the difference is inconsiderable and all *mv* fall within a desirable limit of accuracy.

Table II contains results with the control-pendulum arranged to give a considerable range of absolute times. The constant error of the chronoscope was corrected for 1000. Differences in the length of the periods of magnetization probably induce variations in the constant error.* The average result for longer and shorter times than 1000 is therefore cor-

* K  ple and Kirschmann, as above, p. 149 and p. 169 (Table V).

TABLE II.
CHRONOSCOPIC RECORDS WITH CONTROL-PENDULUM FOR TIMES BETWEEN 18.9σ AND 34 SECONDS.

No.	Position of Bobs.	Rate of Swing.	Position of Contacts.	Av. Res.	MV.	Drop-screen.	
						Av. Res.	MV.
1	Upper bob off. Lower bob raised.	Very fast.	-24.7° , -15.95°	100.5	0.65	100.0	0.55
2	" " "	"	-24.7° , 0°	238.5	0.9		
3	" " "	"	-24.7° , $+24^\circ$	481.5	0.8		
4	" " "	"	-24.7° , $+24^\circ$ 3 oscillations	2034.7	0.77		
5	" " "	"	-24.7° , $+24^\circ$ 5 oscillations	3589.8	0.9		
6	Upper bob off. Lower bob at bottom.	Fast.	-24.7° , $+24^\circ$	543.9	0.63		
7	Upper bob at top. Lower bob at bottom.	Slow.	-24.7° , 0°	439.0	1.0		
8	" " "	"	-24.7° , $+24^\circ$	894.7	1.03		
9	Upper bob at top. Lower bob raised.	Very slow.	-24.7° , $+25^\circ$	2927.1	1.11		
10	Upper bob off. Lower bob raised.	Very fast.	At centre of arc in closest contact	18.9	0.83		
11	" " "	"	1 mm apart	19.3	1.16		
12	" " "	"	2 mm apart	19.5	1.0		
13	" " "	"	3 mm apart	19.5	0.85		
						Av. MV. of 13 series.....	
						Av. MV. of 14 series (Table I).....	
						Av. MV. of 27 series (540 experiments).....	
						Av. MV. 5 series 0.55	
						Av. MV. 6 series 0.68	
						Av. MV. 150 exp. 0.66	
						(120 exp.)	

rect only within a few sigma. It is only the *mv*'s, however, that are under consideration. After the control records with the fall-screen the bobs and contacts were set as given in No. 1 of Table I, and the average result of 100.5σ , *mv* 0.65σ , obtained. The breaking contact was then moved on to 0, and twenty trials gave an average time of 238.5σ , *mv* 0.9σ . Moving the contact to the other end ($+24^\circ$) of the scale increased the absolute time to 481.5σ , *mv* 0.8σ . In the next series, No. 4, the position of the contacts remained the same, but the pendulum was allowed to make three single oscillations before the breaking-contact piece was thrown into operation. The average time recorded was 2034.7σ and the *mv* 0.77σ . In No. 5 the pendulum made five single oscillations, recording 3589.8σ , with 0.9σ *mv*. In Nos. 6, 8, and 9 a single oscillation was measured with varying rates of movement, and in No. 7 a half oscillation with the rate 'slow.' The averages of No. 10 were obtained by placing the contact-pieces as near together as possible at the centre of the arc-scale, while the bobs were arranged for the fastest rate. 18.9σ was therefore the shortest time measurable with this pendulum. Moving the contacts 1 mm apart (No. 11) increased the absolute time by 0.4σ ; a second mm added but 0.2σ (No. 12), while a third had no effect on the average chronoscopic record. These results demonstrate the accuracy with which the contacts may be set to a mark on the scale, as a considerable movement of the contacts on the arc-scale produced no measurable variation in the absolute time.

The average variation of the 260 experiments of Table II is slightly greater than that of the 280 experiments of Table I and also greater than that of the drop-screen control records. No control records were taken during or after the pendulum tests, or an increased variation might have shown some source of increased error in the circuit or chronoscope. But the averages of Table II are within satisfactory limits of accuracy, and the general average variation of 0.70σ for the 540 experiments of Tables I and II when compared with the average *Mv* of 0.66σ from the 120 records with the drop-screen justifies the conclusion that the pendulum is as efficient a control for time-periods between 19 σ and 4 sec. as the screen has been found to be for 100 σ .

We are in a position to compare the accuracy of the Cattell drop-screen and of the pendulum-control with that of the enlarged Krille control-hammer. Külpe and Kirschmann* report an average mean variation of 1.05σ for about 100 chronoscopic records of ten different time-periods between 616σ and 56.6σ . If we assume the same degree of accuracy for the chronoscopes used in the Leipzig and Philadelphia laboratories, the variable error of the screen and of the pendulum is somewhat less than that of the control-hammer; for the average variation of a number of records is a variable error composed of the variable error of the chronoscope and of the variable error of the pendulum, screen, or hammer, as the case may be. In the range of absolute times the pendulum has the advantage; it has practically no upper limit, while the hammer has probably exceeded its maximum at 616σ . The practical advantages of the pendulum as a satisfactory laboratory instrument over the hammer are considerable. The pendulum may be managed during a series of experiments by a simple on-and-off key; it may serve any of the many purposes for which a pendulum may be used in a laboratory, or any pendulum may be fitted with an arc-scale and contacts and serve as a control-instrument (for example, the pendulum of the Cattell light-stimulus apparatus has been thus adapted at Columbia College). On the other hand, the control-hammer requires considerable manual attention in replacing the hammer in position and in resetting two pairs of contacts; it is expensive, cumbersome, and the wear and tear on the instrument from the shock of the falling hammer must in time diminish its accuracy.

The experiments both at Leipzig and at Philadelphia demonstrate a remarkable accuracy of the chronoscope. Langet† determined the probable error of the arithmetical mean of 10 records with the Leipzig chronograph to be 0.03σ , in each of two series. Cattell‡ has pointed out that the variable error of the chronoscope must, from the research of Külpe and

* Külpe and Kirschmann, as above, page 169, Table V, B.

† *Ein Chronograph nebst Control-Apparat für sehr genaue Zeitmessungen*. Phil. Stud., Bd. iv. p. 469.

‡ Am. Journal of Psych., vol. iv. No. 4, p. 597. See also Wundt's reply to this — *Chronoscop u. Chronograph*, Phil. Stud., Bd. vii. No. 4, p. 653.

Kirschmann, be assumed to be about the same as that of the chronograph, so that Lange's determination of the probable error of the latter instrument holds for the chronoscope also. With the control-pendulum, 100σ was recorded in one series with but 0.25σ *mv.* Over $3\frac{1}{2}$ seconds was measured with a mean variation of but 0.9σ . The length of the period of magnetization seems to have no effect on the variable error, and no doubt the latter is in largest part made up of the variable error of the contacts of hammer, screen, or pendulum. The Hipp chronoscope has proved itself a most satisfactory time-piece, and perhaps the same principle might be made use of in a chronoscope that would record with an equivalent degree of accuracy the ten-thousandth part of a second.

DISCUSSION.

THE PHYSICAL BASIS OF EMOTION.

In the year 1884 Prof. Lange of Copenhagen and the present writer published, independently of each other, the same theory of emotional consciousness. They affirmed it to be the effect of the organic changes, muscular and visceral, of which the so-called 'expression' of the emotion consists. It is thus not a primary feeling, directly aroused by the exciting object or thought, but a secondary feeling indirectly aroused; the primary effect being the organic changes in question, which are immediate reflexes following upon the presence of the object.

This idea has a paradoxical sound when first apprehended, and it has not awakened on the whole the confidence of psychologists. It may interest some readers if I give a sketch of a few of the more recent comments on it.

Professor Wundt's criticism may be mentioned first.* He unqualifiedly condemns it, addressing himself exclusively to Lange's version. He accuses the latter of being one of those *psychologischen Scheinerklärungen* which assume that science is satisfied when a psychic fact is once for all referred to a physiological ground.

His own account of the matter is that the immediate and primary result of 'the reaction of Apperception † on any conscious-content' or object is a *Gefühl* (364). *Gefühl* is an unanalyzable and simple process corresponding in the sphere of *Gemüth* to sensation in the

* *Philosophische Studien*, vi. 349, (1891).

† In this article, as in the 4th edition of his *Psychology*, Wundt vaguely completes his *volte-face* concerning 'Apperception' and dimly describes the latter in associationist terms. "Apperception is nothing really separable from the effects which it produces in the content of representation. In fact it consists of nothing but these concomitants and effects. [A thing that 'consists' of its concomitants!] . . . In each single apperceptive act the entire previous content of the conscious life operates as a sort of integral total force" (364, 365), etc. The whole account seems indistinguishable from pure Herbartism, in which Apperception is only a name for the interaction of the old and the new in consciousness, of which interaction feeling may be one result.

sphere of intellection (359). But *Gefühle* have the power of altering the course of ideas—inhibiting some and attracting others, according to their nature; and these ideas in turn produce both secondary *Gefühle* and organic changes. The organic changes in turn set up additional *sinnliche Gefühle* which fuse with the preceding ones and strengthen the volume of feeling aroused. This whole complex process is what Wundt calls an *Affect* or Emotion—a state of mind which, as he rightly says, ‘has thus the power of intensifying itself’ (358–363). I shall speak later of what may be meant by the primary *Gefühl* thus described. Wundt in any case would seem to be certain both that it is the essential part of the emotion, and that currents from the periphery cannot be its organic correlate. I should say, granting its existence, that it falls short of the emotion proper, since it involves no *commotion*, and that such currents *are* its cause. But of these points later on. The rest of Wundt’s criticism is immaterial, dealing exclusively with certain rash methodological remarks of Lange’s; emphasizing the ‘parallelism’ of the psychical and the physical; and pointing out the vanity of seeking in the latter a causal explanation of the former. As if Lange ever pretended to do this in any intimate sense! Two of Wundt’s remarks, however, are more concrete.

How insufficient, he says, must Lange’s explanation of emotions from vaso-motor effects be, when it results in making him put joy and anger together in one class! To which I reply both that Lange has laid far too great stress on the vaso-motor factor in his explanations, and that he has been materially wrong about congestion of the face being the essential feature in anger, for in the height of that passion almost every one grows pale—a fact which the expression ‘white with rage’ commemorates. Secondly, Wundt says, whence comes it that if a certain stimulus be what causes emotional expression by its mere reflex effects, another stimulus almost identical with the first will fail to do so if its *mental* effects are not the same? (355). The mental motivation is the essential thing in the production of the emotion, let the ‘object’ be what it may.

This objection, in one form or another, recurs in all the published criticisms. “Not the mere object as such is what determines the physical effects,” writes Mr. D. Irons in a recent article* which, if it were more popularly written, would be undeniably effective, “but the subjective feeling towards the object. . . . An emotional class is not something objective; each subject to a great extent classifies in this regard for itself, and even here time and circumstance make alteration

* Professor James’s Theory of Emotion, Mind, p. 78, 1894.

and render stability impossible. . . . *If I were not afraid, the object would not be an object of terror*" (p. 84). And Dr. W. L. Worcester, in an article * which is both popularly written and effective, says: "Neither running nor any other of the symptoms of fear which he [W. J.] enumerates is the necessary result of seeing a bear. A chained or caged bear may excite only feelings of curiosity, and a well-armed hunter might experience only pleasurable feelings at meeting one loose in the woods. It is not, then, the perception of the bear that excites the movements of fear. We do not run from the bear unless we suppose him capable of doing us bodily injury. Why should the expectation of being eaten, for instance, set the muscles of our legs in motion? 'Common-sense' would be likely to say that it was because we object to being eaten; but according to Professor James the reason we dislike to be eaten is because we run away" (287).

A reply to these objections is the easiest thing in the world to make if one only remembers the force of association in psychology. 'Objects' are certainly the primitive arousers of instinctive reflex movements. But they take their place, as experience goes on, as elements in total 'situations,' † the other suggestions of which may prompt to movements of an entirely different sort. As soon as an object has become thus familiar and suggestive, its emotional consequences, *on any theory of emotion*, must start rather from the total situation which it suggests than from its own naked presence. But whatever be our reaction on the situation, in the last resort it is an instinctive reaction on that one of its elements which strikes us for the time being as most vitally important. The same bear may truly enough excite us to either fight or flight, according as he suggests an overpowering 'idea' of his killing us, or one of our killing him. But in either case the question remains: Does the emotional excitement which follows the idea follow it immediately, or secondarily and as a consequence of the 'diffusive wave' of impulses aroused?

Dr. Worcester finds something absurd in the very notion of acts constituting emotion by the consciousness which they arouse. How is it, he says, with voluntary acts? "If I see a shower coming up and run for a shelter, the emotion is evidently of the same kind, though perhaps less in degree, as in the case of the man who runs from the bear. According to Professor James, I am afraid of getting wet because I run. But suppose that instead of running I step into a shop and buy

* Observations on some points in James's *Psychology*. II. Emotion.—The *Monist*, vol. III. p. 285 (1893).

† In my nomenclature it is the total situation which is the 'object' on which the reaction of the subject is made.

an umbrella. The emotion is still the same. I am afraid of getting wet. Consequently, so far as I can see, the fear in this case consists in buying the umbrella. Fear of hunger, in like manner, might consist in laying in a store of provisions; fears of poverty in shovelling dirt at a dollar a day, and so on indefinitely. Anger, again, may be associated with many other actions than striking. Shylock's anger at Antonio's insults induced him to lend him money. Did the anger . . . consist in the act of lending the money?" (291). I think that all the force of such objections lies in the slapdash brevity of the language used, of which I admit that my own text set a bad example when it said 'we are frightened because we run.' Yet let the word 'run' but stand for what it was meant to stand for, namely, for many other movements in us, of which invisible visceral ones seem by far the most essential; discriminate also between the various grades of emotion which we designate by one name, and our theory holds up its head again. 'Fear' of getting wet is not the same fear as fear of a bear. It may limit itself to a prevision of the unpleasantness of a wet skin or of spoiled clothes, and this may prompt either to deliberate running or to buying an umbrella with a very minimum of properly emotional excitement being aroused. Whatever the fear may be in such a case it is not constituted by the voluntary act.* Only the details of the concrete case can inform us whether it be, as above suggested, a mere ideal vision of unpleasant sensations, or whether it go farther and involve also feelings of reflex organic change. But in either case our theory will cover all the facts.

Both Dr. Worcester and Mr. Irons are struck by this variability in the symptoms of any given emotion; and holding the emotion itself to be constant, they consider that such inconstant symptoms cannot be its cause. Dr. Worcester acutely remarks that the actions accompanying all emotions tend to become alike in proportion to their intensity. People weep from excess of joy; pallor and trembling accompany extremes of hope as well as of fear, etc. But, I answer, do not the subject's feelings also then tend to become alike, if considered in themselves apart from all their differing intellectual contexts? My theory maintains that they should do so; and such reminiscences of extreme emotion as I possess rather seem to confirm than to invalidate such a view.

In Dr. Lehmann's highly praiseworthy book, 'Die Hauptgesetze des menschlichen Gefühlslebens,' † much is said of Lange's theory; and in

* When the running has actually commenced, it gives rise to *exhilaration* by its effects on breathing and pulse, etc., in this case, and not to *fear*.

† Leipzig, 1892.

particular this same alleged identity of the emotion in the midst of such shifting organic symptoms seems to strike the critic as a fact irreconcilable with its being true. The emotion ought to be different when the symptoms are different, if the latter *make* the emotion; whereas if we lay a primary mental feeling at its core its constancy with shifting symptoms is no such hard thing to understand (p. 120). *Some* inconstancy in the mental state itself, however, Dr. Lehmann admits to follow from the shifting symptoms; but he contrasts the small degree of this inconstancy in the case of 'motived' emotions where we have a recognized mental cause for our mood, with its great degree where the emotion is 'unmotived,' as when it is produced by intoxicants (alcohol, haschisch, opium) or by cerebral disease, and changes to its opposite with every reversal of the vaso-motor and other organic states. I must say that I cannot regard this argument as fatal to Lange's and my theory so long as we remain in such real ignorance as to what the subjective variations of our emotions actually are. Exacter observation, both introspective and symptomatic, might well show in 'motived' emotions also just the amount of inconstancy that the theory demands.

Mr. Irons actually accuses me of self-contradiction in admitting that the symptoms of the same emotion vary from one man to another, and yet that the emotion has them for its cause. How can any definite emotion, he asks, exist under such circumstances, and what is there then left to give unity to such concepts as anger or fear at all (82)? The natural reply is that the bodily variations are within limits, and that the symptoms of the angers and of the fears of different men still preserve enough *functional* resemblance, to say the very least, in the midst of their diversity to lead us to call them by identical names. Surely there *is* no definite affection of 'anger' in an 'entitative' sense.

Mr. Irons finds great difficulty in believing that both intellectual and emotional states of mind, both the cognition of an object and the emotion which it causes, contrasted as they are, can be due to such similar neural processes, viz., currents from the periphery, as my theory assumes. "How," he asks, "can one perceptive process of itself suffuse with emotional warmth the cold intellectuality of another? . . . If perceptions can have this warmth, why is it the exclusive property of perception of organic disturbance (85.)?" I reply in the first place that it is not such exclusive property, for all the higher senses have warmth when 'æsthetic' objects excite them. And I reply in the second place that even if secondarily aroused visceral thrills were the only objects that had warmth, I should see no difficulty in accepting the fact. This writer further lays great stress on the vital difference between

the receptive and the reactive states of the mind, and considers that the theory under discussion takes away all ground for the distinction. His account of the inner contrast in question is excellent. He gives the name of 'feeling-attitude' to the whole class of reactions of the self, of which the experiences which we call emotions are one species. He sharply distinguishes feeling-attitude from mere pleasure and pain—a distinction in which I fully agree. The line of direction in feeling-attitude is from the self outward, he says, while that of mere pleasure and pain (and of perception and ideation) is from the object to the self. It is impossible to feel pleasure or pain *towards* an object; and common language makes a sharp distinction between being pained and having bad feelings towards somebody in consequence. These attitudes of feeling are almost indefinitely numerous; some of them must always intervene between cognition and action, and when in them we feel our whole Being moved (93-96). Of course one must admit that any account of the physiology of emotion that should be inconsistent with the possibility of this strong contrast within consciousness would thereby stand condemned. But on what ground have we the right to affirm that visceral and muscular sensibility cannot give the direction from the self outwards, if the higher senses (taken broadly, with their ideational sequelæ) give the direction from the object to the self? We do, it is true, but follow a natural analogy when we say (as Fouillée keeps saying in his works on *Idées-forces*, and as Ladd would seem to imply in his recent *Psychology*) that the former direction in consciousness ought to be mediated by outgoing nerve-currents, and the latter by currents passing in. But is not this analogy a mere superficial fancy, which reflection shows to have no basis in any existing knowledge of what such currents can or cannot bring to pass? We surely know too little of the psycho-physic relation to warrant us in insisting that the similarity of direction of two physical currents makes it impossible that they should bring a certain inner contrast about.

Both Dr. Worcester and Mr. Irons insist on the fact that consciousness of bodily disturbance, taken by itself, and apart from its combination with the consciousness of an exciting object, is not emotional at all. "Laughing and sobbing, for instance," writes the former, "are spasmodic movements of the muscles of respiration, not strikingly different from hiccoughing; and there seems no good reason why the consciousness of the former two should usually be felt as strong emotional excitement while the latter is not. . . . Shivering from cold, for instance, is the same sort of a movement as may occur in violent fright but it does not make us feel frightened. The laughter excited in children and sensitive persons by tickling of the skin is not neces-

sarily accompanied by any mirthful feelings. The act of vomiting may be the accompaniment of the most extreme disgust, or it may occur without a trace of such emotion" (289). The facts must be admitted; but in none of these cases where an organic change gives rise to a mere local bodily perception is the reproduction of an emotional diffusive wave complete. Visceral factors, hard to localize, are left out; and these seem to be the most essential ones of all. I have said that where they also from any inward cause are added, we *have* the emotion; and that then the subject is seized with objectless or pathological dread, grief, or rage, as the case may be. Mr. Irons refuses to accept this interpretation. The bodily symptoms do not here, he says, when felt, constitute the emotion. In the case of fear they constitute rather the object of which we are afraid. We fear *them*, on account of their unknown or indefinite evil consequences. In the case of morbid rage, he suggests, the movements are probably not the expression of a genuine inner rage, but only frantic attempts to relieve some inward pain, which outwardly look like rage to the observer (80). These interpretations are ingenious, and may be left to the reader's judgment. I confess that they fail to convert me from my own hypothesis.*

Messrs. Irons and Wundt (and possibly Baldwin and Sully, neither of whom accept the theory in dispute, but to whose works I have not access where I write, so that I cannot verify my impression) think that the theory carries with it implications of an objectionable sort philosophically. Irons, for example, says that it belongs to a psychology in which feeling can have no place, because it ignores the self and its unity, etc. (92). In my own mind the theory has no philosophic implications whatever of a general sort. It assumes (what probably every one assumes) that there must be a process of some sort in the nerve-

* Mr. Irons elsewhere says that "an object on being presented suddenly may cause intense fear. On being recognized as familiar the terror may vanish instantly, and while the mental mood has changed, for a measurable time at least, all the bodily effects of the former state are present" (86). Their dying phase certainly is present for a while; but *has* the emotion then 'vanished instantly'? I should rather say that there is then a very mixed emotional state, in which something of the departing terror still blends with the incoming joy of relief. The case of waking from nightmare is for us civilizees probably the most frequent experience in point. On such occasions the horror with me is largely composed of an intensely strong but indescribable feeling in my breast and in all my muscles, especially those of the legs, which feel as if they were boiled into shreds or otherwise inwardly decomposed. This feeling fades out slowly and until it is gone the horror abides, in spite of the fact that I am already enjoying the incomplete relief which comes of knowing that the bad experience is a dream, and that the horror is on the wane. It were much to be wished that many persons should make observations of this sort, for individual idiosyncrasy may be great.

centres for emotion, and it simply defines that process to consist of afferent currents. It does this on no general theoretic grounds, but because of the introspective appearances exclusively.

The objective qualities with which perception acquaints us are considered by psychologists to be results of sensation. When these qualities affect us with pleasure or displeasure, we say that the sensations have a 'tone of feeling.' Whether this tone be due to a mere form of the process in the nerve of sense, as some authors (e.g. Mr. Marshall) think, or to additional specific nerves, as others (e.g. Dr. Nichols) opine, is immaterial. The pleasantness or unpleasantness, once there, seems immediately to inhere in the sensible quality itself. They are beaten up together in our consciousness. But in addition to this pleasantness or painfulness of the content, *which in any case seems due to afferent currents*, we may also feel a general seizure of excitement, which Wundt, Lehmann, and other German writers call an *Affect*, and which is what I have all along meant by an emotion. Now whenever I myself have sought to discover the mind-stuff of which such seizures consist, it has always seemed to me to be additional sensations often hard to describe, but usually easy to identify, and localized in divers portions of my organism. In addition to these sensations I can discern nothing but the 'objective content' (taking this broadly so as to include judgments as well as elements judged), together with whatever agreeableness or disagreeableness the content may come tinged by.* *Such*

* The disagreeableness, etc., is a very mild affection, not drastic or grasping *in se* in the case of any objective content except localized bodily *pain*, properly so called. Here the feeling seems in itself overpowering in intensity apart from all secondary emotional excitement. But I think that even here a distinction needs to be made between the primary consciousness of the pain's *intrinsic quality*, and the consciousness of its degree of *intolerability*, which is a secondary affair, seeming connected with reflex organic irradiations. I recently, while traversing a little surgical experience, had occasion to verify once more the fact that it is not the mere *bigness* of a pain that makes it most unbearable. If a pain is honest and definite and well localized it may be very heavy and strong without taxing the extreme of our endurance. But there are pains which we feel to be faint and small in their intrinsic amount, but which have something so poisonous and non-natural about them that consent to their continuance is impossible. Our whole being refuses to suffer them. These pains produce involuntary shrinkings, writhings, sickness, faintness, and dread. For such emotion superadded to the pain itself there is no distinctive name in English. Prof. Münsterberg has distinguished between *Schmerz* as an original 'content' of consciousness and *Unlust* as due to flexor reactions provoked thereby; and before his Essay appeared, I remember hearing Dr. D. S. Miller and Dr. Nichols maintain in conversation that painfulness may be always a matter of 'intolerability,' due to the reflex irradiations which the painful object may arouse. Thus might even the mildest *Gemütsvorgänge* be brought under the terms of my theory.

organic sensations being also presumably due to incoming currents, the result is that the whole of my consciousness (whatever its inner contrasts be) seems to me to be outwardly mediated by these. This is the length and breadth of my 'theory'—which, as I apprehend it, is a very unpretending thing.

It may be, after all, that the difference between the theory and the views of its critics is insignificant. Wundt admits tertiary feelings, due to organic disturbance, which must fuse with the primary and secondary feelings before we can have an 'Affect'; Lehmann writes: "Constrained by the facts, we are obliged to concede to the organic sensations and tones of feeling connected with them an essential participation in emotion (*wesentliche Bedeutung für die Affecte*") (p. 115); and Professor Ladd also admits that the 'rank' quality of the emotions comes from the organic repercussions which they involve. So far, then, we are all agreed; and it may be admitted, in Dr. Worcester's words, that the theory under attack 'contains an important truth,' and even that its authors have 'rendered a real service to psychology' (p. 295). Why, then, is there such strong opposition? When the critics say that the theory still contradicts their consciousness (Worcester, p. 288), do they mean that introspection acquaints them with a part of the emotional excitement which it is psycho-physically impossible that incoming currents should cause? Or, do they merely mean that the part which introspection can *localize* in the body is so small that when abstracted a large mass of unlocalizable emotion remains? Although Mr. Irons professes the former of these two meanings, the only prudent one to stand by is surely the latter; and here, of course, every man will hold by his own consciousness. I for one shall never deny that individuals may greatly differ in their ability to localize the various elements of their organic excitement when under emotion. I am even willing to admit that the primary *Gefühlston* may vary enormously in distinctness in different men. But speaking for myself, I am compelled to say that the only feelings which I cannot more or less well localize in my body are very mild and, so to speak, platonic affairs. I allow them hypothetically to exist, however, in the form of the 'subtler' emotions, and in the mere intrinsic agreeableness and disagreeableness of particular sensations, images, and thought-processes, where no obvious organic excitement is aroused.*

* Mr. Irons contends that in admitting 'subtler' forms of emotion, I throw away my whole case (88, 89); and Dr. Lehmann enters into an elaborate argument to prove (as he alleges, against Lange and me) that primary feeling, as a possible accompaniment of any sensation whatever, must be admitted to exist (§§ 157-164). Such objections are a complete *ignoratio elenchi*, addressed to some imaginary

This being the case, it seems almost as if the question had become a verbal one. For which sort of feeling is the word 'emotion' the more proper name—for the organic feeling which gives the rank character of commotion to the excitement, or for that more primary pleasure or displeasure in the object, or in the thought of it, to which commotion and excitement do not belong? I myself took for granted without discussion that the word 'emotion' meant the rank feeling of excitement, and that the special emotions were names of special feelings of excitement, and not of mild feelings that might remain when the excitement was removed. It appears, however, that in this assumption I reckoned without certain of my hosts.

Dr. Worcester's quarrel with me at the end of his article becomes almost exclusively verbal. All pleasure and pain, he says, whether primary and of the higher senses and intellectual products, or secondary and organic, should be called 'emotion' (296).^{*} Pleasure or pain revived in idea, as distinguished from vivid sensuous pleasure and pain, he suggests to be what is meant by emotion 'in the sense in which the word is commonly used' (297); and he gives an array of cases in point:

"Suppose that I have taken a nauseous dose and made a wry face over it. No one, I presume, would question that the disagreeableness lay in the unpleasant taste, and not in the distortion of the countenance. Now suppose I have to repeat the dose, and my face takes on a similar expression, at the anticipation, to that which it wore when I took it originally. How does this come about? If I can trust my own consciousness, it is because the vivid reproduction, in memory, of the unpleasant taste is itself unpleasant. . . . If this be the fact, what can be more natural than that it should excite the same sort of associated movements that were excited by the original sensation? I cannot make it seem any more credible that my *repugnance* to a repetition of the dose is due to my involuntary movements than my discomfort in taking it originally was due to the similar movements that occurred then. . . . I hardly think that any one who will consult his own consciousness will say that the reason he likes the taste of an orange is that it makes him laugh or smile to get it. He *likes* it because it tastes good, and is sorry to lose it for the same reason." (*Ibid.*)

theory with which my own, as I myself understand it, has nothing whatever to do, all that I have ever maintained being the dependence on incoming currents of the *emotional seizure* or *Affect*.

^{*} 'The essence of emotion is pleasure and pain,' he adds. This is a hackneyed psychological doctrine, but on any theory of the seat of emotion it seems to me one of the **most** artificial and scholastic of the untruths that disfigure our science. One might as well say that the essence of prismatic color is pleasure and pain. There are infinite shades and tones in the various emotional excitements, which are as distinct as sensations of color are, and of which one is quite at a loss to predicate either pleasant or painful quality.

Now, accepting Dr. Worcester's description of the facts, I remark immediately that the nauseousness and pleasantness are due to incoming nerve-currents—at any rate in the cases which he selects—and the feeling of the involuntary movements as well; so whatever name we give to the phenomena, so far they fall comfortably under the terms of my theory. The only question left over is what may be covered by the words 'repugnance' and 'liking,' which I have italicized, but which Dr. Worcester does not emphasize, as he describes his instances. Are *these* a third sort of affection, *not* due to afferent currents, and interpolated between the gustatory feelings and reactions which are so due? Or are they a name for what, when carefully considered, resolves itself into more delicate reactions still? I privately incline to the latter view, but the whole *animus* of my critic's article obliges me to attribute to him the opinion, not only that the like and dislike must be a third sort of affection not grounded on incoming currents, but that they form the distinctive elements of the 'emotional' state of mind.

The whole discussion sharpens itself here to a point. We can leave the lexicographers to decide which elements the word 'emotional' belongs to; for our concern is with the facts, and the question of fact is now very plain. Must we (under any name) admit as an important element in the emotional state of mind something which is distinct both from the intrinsic feeling-tone of the object and from that of the reactions aroused—an element of which the 'liking' and 'repugnance' mentioned above would be types, but for which other names may in other cases be found? The belief that some such element does exist, and exist in vital amount, is undoubtedly present in the minds of all the rejectors of the theory in dispute. Dr. Worcester rightly regrets the deadlock when one man's introspection thus contradicts another's (288), and demands a more objective sort of umpire. Can such a one be found? I shall try to show now that it possibly has been found; and that Dr. Sollier's recent observations on complete anæsthetics show that in some persons at least the supposed third kind of mental element may exist, if it exists at all, in altogether inappreciable amount.

In my original article I had invoked cases of generalized anæsthesia, and admitted that if a patient could be found who, in spite of being anæsthetic inside and out, could still suffer emotion, my case would be upset. I had quoted such cases as I was aware of at the time of writing, admitting that so far as appearances went they made against the theory; but I had tried to save the latter by distinguishing between the objective reaction which the patient makes and the subjective feeling which it gives him. Since then a number of cases of

generalized anæsthesia have been published, but unfortunately the patients have not been interrogated from the proper point of view. The famous 'theory' has been unknown to the reporting doctors. Two such cases, however, described by Dr. Berkley of Baltimore,* are cited by Dr. Worcester 'for what they are worth' in its refutation (294). The first patient was an Englishwoman, with complete loss of the senses of pain, heat and cold, pressure and equilibrium, of smell, taste, and sight. The senses of touch and of position were not completely gone, but greatly impaired, and she could hear a little. As for visceral sensations, she had had no hunger or thirst for two years, but she was warned by feeling of the evacuative needs. She laughs at a joke, shows definitely grief, shame, surprise, fear, and repulsion. Dr. Berkley writes to Dr. Worcester as follows: "My own impression derived from observation of the patient, is that all mental emotional sensibilities are present, and only a little less vivid than in the unanæsthetic state; and that emotions are approximately natural and not at all coldly dispassionate."

The second case was that of a Russian woman with complete loss of cutaneous, and almost complete loss of muscular, sensibility. Sight, smell, hearing preserved, and nothing said of visceral sensation (in Dr. Worcester's citation). She showed anger and amusement, and not the slightest apathy.

This last case is obviously too incompletely reported to serve; and in the preceding one it will be noticed that certain degrees of visceral and of muscular sensibility remained. As these seem the important sorts emotionally, she may well have felt emotion. Dr. Berkley, however, writes of her 'apathy'; and it will be noticed that he thinks her emotions 'less vivid than in the unanæsthetic state.'

In Dr. Sollier's patient the anæsthesia was far more complete, and the patient was examined for the express purpose of testing the dependence of emotion on organic sensibility. Dr. Sollier, moreover, experimented on two other subjects in whom the anæsthesia was artificially induced by hypnotic suggestion. The spontaneous case was a man aged forty-four; the hypnotic cases were females of hysteric constitution.† In the man the anæsthetic condition extended so far that at present every surface, cutaneous and mucous, seems absolutely insensible. The muscular sense is wholly abolished; the feelings of hunger and satiety do not exist; the needs of defecation and micturition

* Brain, Part IV, 1891.

† The paper, entitled 'Recherches sur les Rapports de la Sensibilité et de l'Émotion,' will be found in the *Revue Philosophique* for March of this year, vol. XXXVII, p. 241.

are unfelt; taste and smell are gone; sight much enfeebled; hearing alone is about normal. The cutaneous and tendinous reflexes are lacking. The physiognomy has no expression; speech is difficult; the entire muscular apparatus is half paralyzed, so that locomotion is almost impossible.

"'I know,' this patient says, 'that I have a heart, but I do not feel it beat, except sometimes very faintly.' When an event happens which ought to affect it [the heart, as I understand the text], he fails equally to feel it. He does not feel himself breathe, or know whether he makes a strong or a weak inspiration. 'I do not feel myself alive,' he says. Early in his illness he several times thought himself dead. He does not know whether he is asleep or awake. . . . He often has no thoughts. When he does think of anything it is of his home or of the war of 1870, in which he took part. The people whom he sees come and go about him are absolutely indifferent to him. He does not notice what they do. 'They do not appear,' he says, 'like natural men to me, but more like mechanisms.' Similar perturbations of perception occur also in hearing. 'I do not hear in the old way; it is as if it sounded in my ear, but did not enter into my head. It does not stay there long.' His *aprosodia* is complete, and he is incapable of interest in anything whatever. Nothing gives him pleasure. 'I am insensible to everything; nothing interests me. I love nobody; neither do I dislike anybody.' He does not even know whether it would give him pleasure to get well, and when I tell him that his cure is possible it awakens no reaction—not even one of surprise or doubt. The only thing that seems to move him a little is the visit of his wife. When she appears in the room 'it gives me a stroke in the stomach,' he says; 'but as soon as she is there I wish her away again.' He often has a fear that his daughter may be dead. If she should die I believe I should not survive her, although if I never were to see her again it would make no difference to me.' His visual images are non-existent, and he has no representation of his wife when she is gone. The weakness of the sensations remaining to him gives him a sense of uncertainty about all things: 'I am never sure of anything.' Nothing surprises or astonishes him. His state of apathy, of indifference, of extreme emotionlessness, has developed slowly *pari passu* with the anæsthesia. His case realizes, therefore, as completely as possible the experiment desiderated by W. James."

In the hypnotic experiments, Dr. Sollier provoked in his subjects sometimes visceral and sometimes peripheral anæsthesia, and sometimes both at once. He registered the organic reactions (by pneumograph, etc.) as far as possible, and compared them with those produced in the same subject when an emotion-exciting idea was suggested, first in the anæsthetic and then in the normal state. Finally, he questioned the subject on the impressions she had received. For the detailed results the reader must consult the original paper. I will only mention those which seem most important, as follows:

(1) Complete peripheral anaesthesia abolishes completely the power of movement. At the same time the limbs grow cold and sometimes blue (247).

(2) When visceral anaesthesia is added, the patient says she feels as if she no longer were alive (*ibid*).

(3) When totally anaesthetic she feels no normal emotion whatever at the suggestion of hallucinations and delusions which have the power of moving her strongly when the sensibility is restored. When the anaesthesia is less complete she may say that she feels not the usual emotion, but a certain stroke in the head or stomach at the reception of the moving idea (250, 254).

(4) When the anaesthesia is solely peripheral, the emotion takes place with almost normal strength.

(5) When it is solely visceral, the emotion is abolished almost as much as when it is total, so that the emotion depends almost exclusively on visceral sensations (258).

(6) There is sometimes a very slight motor reaction shown by the pneumograph in visceral anaesthesia when an exciting idea is suggested (Figs. 2, 7 *bis*), but M. Sollier thinks (for reasons of a highly speculative kind) that in complete *inmotivity* the visceral reactions themselves do not take place (265).

The reader sees that M. Sollier's experimental results go on the whole farther than 'my theory' ever required. With the visceral sensibility not only the 'coarser' but even the 'subtler' forms of emotion depart. Some people must then be admitted to exist in whom the amount of supposed feeling that is not due to incoming currents is a negligible quantity. Of course we must bear in mind the fallibility of experiments made by the method of 'suggestion.' We must moreover remember that the male patient's *inmotivity* may have been a co-ordinate result with the anaesthesia, of his neural lesions, and not the anaesthesia's mere effect. But nevertheless, if many cases like those of M. Sollier should be found by other observers, I think that Prof. Lange's theory and mine ought no longer to be treated as a heresy, but might become the orthodox belief. That part, if there be any, of emotional feeling which is not of afferent origin should be admitted to be insignificant, and the name 'emotion' should be suffered to connote organic excitement as the distinctive feature of the state.

WILLIAM JAMES.

HARVARD UNIVERSITY.

PSYCHOLOGICAL LITERATURE.

Introduction à la psychologie expérimentale. A. BINET. Paris, Alcan, 1894. Pp. 146.

It is to be regretted that M. Binet has not seen fit to lead the readers of the present volume to anything like a thorough and systematic grasp of the elements of experimental psychology. At a time when the scientific character of psychology is so much urged and disputed in France and America, it is of extreme importance that every contribution to the didactic literature of the subject should indicate the progress made towards enstating it in the high place claimed for it by its advocates—and by none more forcibly than M. Binet himself.

The volume opens with a long and circumstantial description of several of the more prominent laboratories in Europe, including a list of apparatus in the one at Paris, and a bibliography of the studies performed there. These details, which are of little interest to the novice, and which the more advanced worker would expect to find in monographs of a different character, might better have been relegated to a preface or appendix. After a short discussion of psychological methods, the author proceeds to give a few of the more striking and unusual experiments under sensation, attention, etc. These have the effect of arousing interest in the subject. Yet they will not, it is feared, impress the novice with the idea that the subject in hand is exact, natural, and capable, within ever-widening limits, of yielding precise numerical results. The student who takes up experimental psychology is not a child, and his wonder has no need to be stimulated in order to lead him onward in the pursuit. It would seem preferable, therefore, to subordinate these few startling and isolated facts to the many carefully-studied, interwoven phenomena which furnish the bulk of the science. Such a detailed treatment, while it may lack the literary beauty of a popular essay, need not, I believe, lessen its interest to the beginner, while it would add immensely to its value as a presentation of the elements of the newer psychology. The chapter on Movement is to be commended, on account of its appreciative treatment of that side of psychology, and its indication of the direction which recent research

in this line has taken. But even here too much attention is bestowed on particular apparatus, and on special workers and studies. The treatment of association (under Ideation) and psychometry are more satisfactory; of special interest is the discussion of the uses and meaning of the time measurements; yet here again one might wish for more figures and definite results; for though the absolute times, as M. Binet concludes, may not have any great significance or universal application, the *relative* length of the distinct phases of an act, and of different acts, are the most important determinations in that branch, as yet. In a chapter on Observation, the author devotes considerable space to a discriminating defence of the statistical method employed with such effect by Galton and others.

One cannot help confessing to a feeling of disappointment when it is considered what even a short book like this might have been and how much it might have accomplished for the science,—coming, as it does, from one amply qualified to judge the relative importance of experiments, and to give any number of them, so chosen as to arouse the deepest interest and, at the same time, picture accurately the present status of experimental psychology.

H. C. WARREN.

PRINCETON COLLEGE.

Practical Lessons in Psychology. BY WILLIAM O. KROHN. Chicago, The Werner Company, 1894. Pp. 402.

This work is best described in the preface of the author. It originated in 'lectures on psychology, chiefly on those phases that must and do come in for a large share of consideration on the part of every successful teacher'; and these, having first been 'delivered at various institutes and other gatherings of teachers,' are now collected into book form, with little change in plan or manner of expression, but with the addition of considerable new material. It is, therefore, only a 'collection of personal letters' designed to 'develop *tact* on the part of the teacher,' to assist him in 'ministering to the wants of the growing child-nature,' and to interest him in child-study alike for the purposes of science and of practice.

This twofold aim—psychological and pedagogical in one—is shown in the divisions of the volume. Of the twenty-six chapters or 'lessons' two may be described as introductory; four are on the brain and nervous system; eight discuss sensation, including the development of the senses and sense-illusions; three are devoted to association, memory, and imagination; while the fifteenth takes up 'The Contents of the Child's Mind on Entering School'; the twenty-fifth, methods of testing and measuring the mental faculties, especially in school-children; the twenty-sixth, child-study; and an appendix by Miss Cary of Cham-

paign, Illinois, commends the principles and methods of kindergarten instruction to the educational public.

Professor Krohn writes in full sympathy with the recent developments of psychological science, and from the standpoint of a wide acquaintance with the latest results of physiological and experimental inquiry. He shares the conviction of many other workers in the same field, that the new science is fitted to furnish not only a sound basis for pedagogics, but also a needed corrective to much that is useless or even harmful in our present educational system. His work in its published form, therefore, will excite the same interest in the mind of the class to which it is addressed as it has already aroused when delivered from the lecture-platform. Further, it will serve to give many teachers a preliminary introduction to the newer forms of psychological and pedagogical science. But this, unhappily, marks also the limit of its influence. The selection of topics and the manner of treatment both display the effects of popular discourse in a way that is often tantalizing in the extreme. Sometimes the desire for vividness and attractiveness leads the author not only into statements of doubtful psychological value, but also into an imperfect use of his pedagogical opportunities. It is doubtful, for instance, whether the uninstructed reader will gather a clear understanding of the phenomena of aphasia, agraphia, and alexia from the discussions of p. 78 *et seq.*, even though the subject is undoubtedly calculated to enforce the fact of the correlation of mind and brain. The treatment of memory and imagination, again, in Lessons XIX-XXI is confused even to the student of psychology; and the omission of all the thought-functions except reasoning neglects the capital chance of driving home the author's excellent remarks on the importance of the imagination in intellectual development.

The careful reader will also notice the lack of reference to the sources of the plates, though many of them are old friends, and the entire absence of an index.

A. C. ARMSTRONG, JR.

WESLEYAN UNIVERSITY.

Man and Woman: a Study of Human Secondary Sexual Characters.
HAVELOCK ELLIS. Illustrated. London and New York, Charles
Scribner's Sons, 1894. Pp. 409.

There is a keen general interest in the problem with which this book is concerned—the real differences between the sexes, and the light thrown by them upon the possible future position of woman in social and political life. The author has had such questions distinctly in view through his long and conscientious study of his theme; but at

its close he candidly acknowledges that the mass of material he has brought together, however valuable for many purposes, does not answer the inquiries for which many would turn to his volume.

"Our investigation shows us in what state of mind we ought to approach the whole problem; it can scarcely be said that it gives us the definite solution of definite problems. It is not on that account fruitless. It is something to have asked the right question and to have been put on the right road." No doubt of this; and a brief synopsis of Mr. Ellis' work will show that he has approached the subject from the right side.

The earlier chapters are devoted to the secondary physical differences of men and women—such as the growth and proportions of the body, the construction of the pelvis, the skull, and the brain in the two sexes; the variations which they exhibit in the acuteness of the special senses, in muscular power and manual dexterity. The crucial question of the intellectual contrasts between the sexes he enters into at length, with inconclusive expressions.

"It cannot be said that in this chapter we have reached any very definite results. A few careful experiments, which need confirmation and extension; a certain number of observations on irregular masses of data, accumulated in the practical experiences of life, which have their value, although they are open to various misinterpretations,—this is about all that experimental psychology has yet to show us in regard to the intellectual differences of men and women."

Positive differences between the sexes are the quicker reaction to physical and psychic stimuli in women—called by our author 'affectability'; their much less spontaneous and pronounced artistic impulses; their decidedly inferior tendency to variability in both the physical and intellectual fields; and their greater rapidity of growth, countervailed by a tendency not less distinct to an earlier arrest of development, which traits are quite as marked in their mental as in their bodily life. Women have the 'infantile diathesis.' They share their special characteristics with children. "We have found over and over again that, when women differ from men, it is the latter who have diverged, leaving women nearer to the child-type."

This sounds ominous for the fair sex. Immature in their essential differences, children all their lives, how can they claim equality with man? But with a *tour de force*, such as he displays in several critical conjunctures, Mr. Ellis saves his science and his chivalry at once. The child, the infant, in fact, alone possesses in their fulness 'the chief distinctive characters of humanity.' "The highest human types, as represented in men of genius, present a striking approximation to the child-type." "In man, from about the third year onward,

further growth is to some extent growth in degeneration and senility." Hence the true tendency of the progressive evolution of the race is to become child-like—to become feminine.

"It would not be difficult to multiply examples of the ways in which women are leading evolution. In the saying with which Goethe closed his 'Faust' lies a biological verity not usually suspected by those who quote it."

All will admire the spirit of fairness which pervades Mr. Ellis' work. He has no hobby to ride, no pet theory to sustain; and his frank acknowledgment at the close, that "we have not reached the end proposed at the outset," will invest his extensive collation of facts with the more value to students from the certainty that they are presented with an unprejudiced mind; and as he rightly says that a man and a woman 'are the two most interesting beings in the world,' his book cannot fail to please and instruct a large number of readers.

DANIEL G. BRINTON.

UNIVERSITY OF PENNSYLVANIA.

The Psychic Factor: an Outline of Psychology. C. VAN NORDEN.
New York, D. Appleton & Co., 1894. Pp. vii, 223.

This is a text-book written from an objective point of view. An elementary book of this kind is needed; and it is to the credit of Mr. Van Norden that he has anticipated the original workers in the field of the borderland of mental science in presenting an outline of their more striking results, which is, as far as it goes and in the main, accurate. The author devotes more than half of his book to the results of such 'objective' (i.e., pathological, comparative, genetic) results under the caption 'Mind in General.' Some of the sections are Mind in Plants ('Living matter is always psychic'), Mind in Animals (from a thorough-going genetic and evolution point of view), Subconsciousness, Sleep, Somnambulism, Hypnosis, Thought-transference, Lucidity, Hysteria, Criminality. The treatment of these topics is generally well informed, but dogmatic, on a basis of too little literature; although the author's authorities are generally well chosen. The rest of the book is an attempt to say as little as possible of the psychology of consciousness—the subjective point of view. 'The Feelings' are given five pages, and 'Willing' four, under the general caption 'Mind in Detail.' The treatment of the intellectual operations is about as inadequate. The book will prove useful, doubtless, for supplementary reading in connection with an elementary work in general psychology, in the hands of teachers who know their authorities; but it is in no sense an 'Outline of Psychology,'—whether it be or be not a good treatise on what the

author privately means by the barbarous major title,—and should not be substituted for a text-book on academic psychology. J. M. B.

Classification objective et subjective des arts, de la littérature, et des sciences.

R. DE LA GRASSERIE. Paris, Alcan, 1894.

The author proposes a classification which includes not only the sciences, but also the arts and literature, i.e., all the manifestations of the mental activity of man. We are able to notice only two points from this compact volume: the place given to psychology and the author's examination of the classification of Wundt.

I. Wundt proposed a new classification in the *Philosophische Studien* (1888), the last before this of M. Grasserie. He criticised Comte and Spencer, holding that Comte gave an inadequate recognition to sociology and still less to psychology. This is surprising to those who are familiar with the work of Comte: for he was the first to propose the term Sociology, and he devoted one of the six volumes of his *Cours* to the discussion of this science, advocating its place.

As to Spencer, Wundt charges him with attaching psychology to biology and with giving too great place to sociology. Further, Wundt claims that both Comte and Spencer were quite wrong in eliminating philosophy, or metaphysics, entirely from the body of the sciences. In this connection we may recall the *Discours sur l'organisation des sciences philosophiques*, in which Jouffroy, the psychologist of pure eclecticism, holds that the sciences must separate from metaphysics in proportion as they discover their own proper methods of research. Wundt's principle of classification is that the world and the mind, despite their apparent opposition, are not really distinct, or at least are not universally recognized as such. On this ground he distinguishes the 'formal,' or abstract, from the 'real' sciences. The latter are further divided, following the formula of Bentham, Ampère, and Stuart Mill, into Sciences of Nature and Sciences of Mind. To the Sciences of Mind psychology belongs, and it has several branches: (1) *Psychology of Animals*, which deals with the presence of consciousness in the phenomena of life; (2) *Race Psychology*, which investigates consciousness in different peoples and races; (3) *Psychology Proper*, which studies the relation of mental life to certain bodily processes; (4) *Psychology as Natural History*, which inquires into the development of humanity, on the basis of ethnology and anthropology. Finally, dominating both the domain of these different sciences and that of morals, art, and religion, Wundt places *Philosophical Psychology*, or the philosophy of mind considered apart from all this material: the fundamental and synthetic idea of the life of mind. Thus considered, psychology is the general foundation of all the mental sciences.

II. M. de la Grasserie's classification is drawn from all that precede him. He divides the manifestations of intellectual activity into three groups: (1) the arts and literature; (2) the sciences of nature; (3) the so-called sciences of man. Psychology belongs to the third group, and beside it are law and political economy. It unites with anthropology on the physiological side; but it is at the opposite extreme from the group of studies formerly designated by the vague word Philosophy. Metaphysics is an inquiry into the origin and end of the world and man: it escapes the method of direct observation. But psychology is quite different. It is the science of the soul according to some; of the cerebral functions according to others. In either case it is a science of functions and phenomena.

For a long time psychology has vainly attempted to constitute itself deductively or *a priori*. To-day it proceeds inductively and rests upon directly observed facts like other sciences. Psychology includes: (1) The psychology of the facts of our lower activity—those which are less intense in proportion as we descend in the scale of beings. This study is destined to explain the phenomena of the unconscious life of man. (2) Morbid psychology, which studies the abnormalities of human activity. This science is still in process of formation, having been hindered by the theories of charlatans. It studies, in acute forms, the phenomena of which normal psychology studies the regular forms. (3) Normal psychology: the study of the mental functions in all their manifestations as the old psychology studied them. But it frees itself from the dogmatism of the old and its metaphysical method. The new psychology proceeds scientifically.

J. PHILIPPE.

SORBONNE, PARIS.

Hegel's Philosophy of Mind. Translated from the Encyclopædia of the Philosophical Sciences, with five Introductory Essays. W. WALLACE. Oxford, Clarendon Press. 1894. Pp. cciv, 202.

This new work from the pen of the eminent English exponent of Hegel has a twofold value. The translation itself presents, with the well-known excellence of Prof. Wallace's earlier version of the 'Logic,' the third and concluding part of Hegel's 'Encyclopædia.' Students of psychology should be very grateful for a work which is itself extraordinarily suggestive on many modern points of view, coming as it does in a translation which is itself an interpretation.

The 'Five Introductory Essays' occupy more than half of the work. They cover many topics of current interest, and give Professor Wallace's reflections on modern psychology. The titles are: 'On the Scope of a Philosophy of Mind,' 'Aims and Methods of Psychology,'

'On Some Psychological Aspects of Ethics,' 'Psycho-genesis,' 'Ethics and Politics.' These essays are discursive, sometimes grievously repetitious, always suggestive, occasionally valuable, i.e., as stating a point of view in exposition or criticism with luminous clearness. Their greatest value, I think, resides in the fact that they show the essential sympathy of thinkers of the idealistic school with the natural-history method, and with the experimental method, as well, now so zealously advocated by competent psychologists. And with this goes the implied rebuke which the work itself will administer to some who are only psychologists. Some, I venture to say, will read Wallace's part of the work without reading Hegel's. Raillery at metaphysics, and especially at the Hegelian metaphysics, has been indulged in by many men who do not see that such talk only betrays ignorance of philosophical system and absence of philosophical culture. For it is safe to say that Hegel's influence in bringing the philosophical thought of the time up to the freedom of genetic and comparative research in psychology is due as much to Hegel as to the much-talked-of but little-read Herbart—I personally should say more. Let us remember that, however successful we hope the attempt will be to throw off the bonds of crude and incompetent speculation, yet psychology can never place herself outside the organic development of thought in the sphere of the humanities. If she should succeed in this it would only render her own independence temporary and unfruitful, and she would lose her opportunity of service to modern culture and life. The permanent justification of all science is philosophy; and it is philistinism in spirit and fatuity in policy to secure independence of the mother discipline by a hasty attempt to stab her in the back.

A great defect of Professor Wallace's essays is what I may call their 'personal ambiguity.' Possibly it is because Plato set so flagrant an example of it that all the writers of Professor Wallace's coterie prefer to conceal their personality behind a screen of history. To any one not familiar with Hegel's treatise—I had almost said, *in the German*—the essays in this book will be extremely ambiguous in respect to what is Hegel's and what is Wallace's. The section on Hypnotism is an example (pp. clxiv ff.). Surely it is not fair that a reader should be obliged first to read Wallace as a help to Hegel, then to read Hegel to find out the faithfulness of Wallace, and then to construct a calculus of difference on the basis of wider reading of both Hegel and Wallace. It is all very well to discourse about the continuity of historic insight; but in psychology we emphasize doctrines more than insight, and we want to know both who our teachers are and when they were born.

J. M. B.

SENSATIONS OF THE SKIN.

The Relative Sensitivity of Men and Women at the Nape of the Neck
(By Weber's Test). FRANCIS GALTON. *Nature*, May 10, 1894.

Mr. Galton has tabulated the results of Weber's test upon 932 men and 377 women, with the object of presenting whatever characteristic differences between the sexes might appear. The results were gathered at Mr. Galton's laboratory in London, and may be regarded as pertaining to the general public, or at least such portion of the public as would be sufficiently interested in the matter to submit to the test. The nape of the neck was chosen because it was a portion of the skin, not used or worn, in which all persons would be equally unpractised, which could not be seen, and which was of fairly coarse sensitivity. The record consists of the minimum distance in millimetres between the compass-points which when applied could be perceived as two points. The smallest perceptible interval for the average man is 13.8 mm; for the average woman, 11.8. In general, then, "the average delicacy of female discrimination between the two points is to that of the male in a ratio that lies somewhere between 7 to 6 and 8 to 7, or thereabouts." The question of the distribution of men and women in this respect is presented with the exactness and ingenuity in statistical method that characterizes Mr. Galton's contributions. It appears that the variability of women is greater than that of men, and again in a ratio of about 8 to 7. This variability may be a physiological fact, or it may be a psychological fact due to the greater variability in the powers of attention and observation required in the test. The latter factor, in the author's view, is at least a partial cause of the difference in question.

Mr. Havelock Ellis, in his recent work on *Man and Woman*, has indicated the bearing of researches of this kind upon the general view of secondary sexual differences; and the present results form a valuable contribution to the anthropometry of the tactile sensitivity.

JOSEPH JASTROW.

UNIVERSITY OF WISCONSIN.

FUNCTIONS OF THE INTERNAL EAR.

Funktion und Funktionsentwicklung der Bogengänge. KARL L. SCHAEFFER. *Zeitsch. f. Psych.*, VII. 1-9. 1894.

Akustische Versuche an einer labyrinthlosen Taube. W. WUNDT. *Phil. Stud.*, IX. 496-509. 1894.

Schaeffer's article is in continuation of his whirling-table experiments described in part in Bd. III. pp. 185-192 of the *Zeitsch. f.*

Psychologie. He there showed that invertebrates, i.e., animals without the labyrinth, were not made dizzy by being whirled on the table. The present article promises a publication of copious results confirming the previous investigation, and in addition describes an *experimentus crucis* on tadpoles from which it appears that whirling produced no effects of 'manège' motion on these animals until the semicircular canals had become developed. Symptoms of giddiness appeared as soon as the formation of the canals was completed. The article is prefaced by a scanty history of the development and literature of the problem. Curiously enough Ewald's research on the eighth nerve is not mentioned.

In vol. VIII. of the Phil. Stud., Wundt discussed some experiments (Scripture, Phil. Stud. VIII. p. 638; Cross and Goodwin, Proc. Am. Acad. Arts and Sciences, XXVII. 1891) on the direct excitation of the auditory nerve by sound-waves, with especial reference to their bearing on the doctrine of specific energy of the nerves. Among other facts in support of his views Wundt mentioned the experiments by Ewald on the hearing powers of pigeons from which both labyrinths had been extirpated. As a result of this Ewald sent Wundt a pigeon from which the labyrinths had been removed, with the condition that if Wundt should become convinced that the pigeon could hear he should give some public expression to his conviction. The present article is practically the 'protocol' of the experiments on Ewald's pigeon and a normal 'standard' pigeon. The sounds used were clangs, compound clangs, and noises (rapping on the door, electric bells, etc.). The birds were observed in a dim light by means of a telescope placed in the adjacent room. First Ewald's pigeon was experimented upon by a series of stimuli occurring at intervals of two minutes, and then the normal bird was subjected to a like series. The 'reactions' consisted in sudden starts, movements of the head, of the eyes, and of the eyelids. With the exception that Ewald's bird gave but doubtful signs of being affected by tones above 440 vibrations, there was little difference in the general way in which the pigeons 'reacted' on the sounds. Both birds reacted on a majority of the stimuli, and both often failed to react, especially when a stimulus had already been several times repeated. To meet the objection that reactions were in response to sensations of touch, the tympana of Ewald's bird had been removed. Moreover, for both birds the sound-stimuli were produced in an adjacent room, while both failed to react on violent disturbances of the air from bellows worked immediately beneath the cages.

After completion of Wundt's experiments, Ewald's pigeon was handed over to Dr. Held, Docent in Anatomy in Leipzig, for microscopic examination of the labyrinths and adjacent parts. Held found that the semicircular canals had either disappeared or were without

nerve-tissue. There were no traces of pus and the cerebellum was uninjured.

In how far the pigeon deprived of labyrinths perceived sound it is almost impossible from the nature of the reactions to say; but the same may be said in regard to the normal bird.

The facts that the reactions increased with the intensity of the sounds and that reactions on a new sound followed inaction after a series of like sounds would seem to indicate a perception of quantitative and qualitative differences in the stimuli. But at any rate this evidence, together with the experiments on the intercranial conduction of tones, make it highly probable that the action of the labyrinth is not essential to the perception of sound.

F. ANGELL.

STANFORD UNIVERSITY.

THE PERCEPTION OF DISTANCE.

Das Verhältnis von Accommodation und Konvergenz zur Tiefenlokalisation. FRANZ HILLEBRAND. Zeitsch. f. Psychol., VII. 97-151. 1894.

This article reports the following experiments: An apparatus is so arranged that the subject, with one eye closed, looks with the other through a tube whose further end is closed by a diaphragm with an opening measuring 1 cm \times 1.5 cm; and fixates the sharply-cut vertical edge of a piece of black cardboard which fills up one half of the field of view, the other half being occupied by a brilliantly-illuminated white glass plate, which forms a background at a distance of a little more than one meter from the eye. The black cardboard can be moved smoothly backward and forward, its edge retaining constantly the same position in the field of view. By this arrangement all 'empirical' factors—varying size of retinal image, double-images on the two retinae, etc.—which might reveal the distance of the edge fixated are excluded, and it becomes possible to determine the influence of accommodation and of the convergence which accompanies it even when one eye is closed. Experiments performed under these conditions showed:

1. That when the fixated object was moved backward or forward within the range of easy accommodation, and with a rapidity which allowed it to be constantly accommodated for, it was impossible to tell with any accuracy whether the object was moved nearer or farther away.
2. That when the fixated object changed its distance so suddenly that it was impossible to follow it with accommodation, each observer was able to tell accurately in which direction the movement had taken place, if the difference in distance was sufficiently great.

In another series of experiments, in place of the black cardboard was substituted another cardboard with an adjustable diaphragm which entirely concealed the white background except so much as could be seen through the diaphragm. If now the opening remained unchanged in distance but was made smaller or larger, the observer obtained the impression of an increase or decrease in distance, although accommodation and convergence did not alter. If the cardboard was moved nearer, and the opening at the same time was more than proportionally diminished, then in spite of increasing strain of accommodation the observer believed that the object had been moved farther away.

From these results Dr. Hillebrand concludes that accommodation and convergence, when thus isolated, give us no information in regard to the distance of the point of fixation; in other words, that 'so-called muscle-sensations' have no existence, or at least have no importance in visual localization. Without them it is easy to explain the results mentioned under (2) above, on the supposition that when the change in distance is sudden the observer voluntarily changes his accommodation, and knows, since it is voluntary, whether the new accommodation is for a nearer or farther object; and hence knows also whether the object has moved nearer or farther away, because if his first change of accommodation has not succeeded in making the image of the object clearer, a second change in the opposite direction will do so. When, however, the change in accommodation is involuntary, as in the experiments under (1), there is no means of knowing the direction or even the fact of movement, since, as these experiments establish, the changes in accommodation and the always accompanying changes in convergence yield no muscle-sensations to reveal the nature or the existence of these changes.

E. B. DELABARRE.

BROWN UNIVERSITY.

ASSOCIATION, REACTION.

Minor Studies from the Psychological Laboratory of Cornell University.

Communicated by E. B. TITCHENER. I. 'Mediate' Association.

H. C. HOWE. II. 'Sensorial' and 'Muscular' Reactions. A. R.

HILL and R. WATANABE. *Am. Jour. of Psychol.*, vi. 239-246.

Jan. 1894.

Professor Titchener contributes the first-fruits of the Psychological Laboratory of Cornell University, from which much admirable work in experimental psychology may be expected. Mediate association and sensory and muscular reactions seem to be receiving more than their due share of attention just now, but these experiments are presented in a concise form which should serve as a model.

Mr. Howe repeated and varied Dr. Scripture's experiment on mediate or subconscious association, and found (as Prof. Münsterberg had previously found) that he could not confirm Dr. Scripture's results. Dr. Scripture's experiment consisted in presenting for a short time words with obscure signs, which latter the observer was supposed not to notice distinctly nor remember. When one of the words was presented a second time, a word having the same sign was likely to recur. The present writer remarked in *Mind* some years ago that subconscious association seems to play an important part in the sequence of ideas. When an observer is asked to name the first word suggested by a given word, the word actually named seems often to be connected with the original word by links which are not given in distinct consciousness. But the process is one difficult to observe or to study by experiments which are unambiguous.

Messrs. Hill and Watanabe made experiments on sensory and motor reactions in which the movement was made with the lips and with the closed thumb and index-finger. They found the distinction to obtain in these cases as well as when the movement was made in the usual fashion. The Wundt controlling hammer was out of order, and the times and differences are said not to be absolute, but in most cases the sensory times were 78-155 σ longer than the motor times. With some of the observers a distinction between motor and sensory reactions could not be found and the writers conclude that "not every person is able to function as a reaction-subject. Rather is there required for the work a special kind of mental disposition or *Anlage*." The present writer does not consider the interpretation of motor and sensory reactions given by Professor Wundt and Dr. Lange as valid, but—as the President of the University from which these studies come has remarked—a man has before this gone out to look for asses and found a kingdom. In the manner suggested independently by Professor Flournoy and Professor Baldwin the attitude of the subject in reacting may throw light on the important distinction between visual, auditory, and motor 'types.'

Ueber den Einfluss der Geschwindigkeit des Pulses auf die Zeitdauer der Reactionszeit bei Schalleindrücken. J. J. VAN BIERVLIET. Philos. Stud., x. 160-167. 1894.

Professor van Biervliet describes with unusual clearness experiments made with unusual exactness on the relation between the rate of the pulse and the length of the reaction-time. In the case of 10 of the 11 observers the reaction-time tended to become shorter as the pulse was quicker. Thus with one observer the following results were obtained:

Pulse	70 to	80	Reaction-time	130σ
	80	90		126σ
	90	100		121σ
	100	110		117σ

In other cases there is less regularity, but the number of experiments and careful methods justify the conclusion that "in general the reaction-time for sound is lessened as the rate of the pulse increases." The reactions were 'sensory,' but the times and their variability are about normal, and it does not seem possible that they could be much reduced by directing the observer to make the reactions 'motor.' These 11 observers and the 8 tested by Dr. Dessoir must be counted among those who have not the *Anlage* necessary for experiments on sensory and motor reactions.

Mediate Association. W. G. SMITH. *Mind*, N. S. III. 289-304. July, 1894.

The first place in the July No. of *Mind* is given to experiments made by Dr. Smith at Leipzig and used in an inaugural dissertation. Dr. Scripture's experiments were once more repeated, and a variation was introduced by partly memorizing the series. The result may be readily summed up in the words of the author. "Münsterberg's results were entirely negative; so are those of the present research." While Dr. Smith's paper does not contain any important advance in experimental method, the discussion is timely and interesting. The connection of non-contiguous terms in a series which makes it easier to learn the alternate terms when the whole series has been previously memorized (Ebbinghaus) is attributed to motor activity, and Hume's reference to indirect association is quoted and criticised. We may expect further experiments on mediate association from the Yale Laboratory which will clear up discrepancies between Dr. Scripture's original experiments and the results of later investigations.

Ein Beispiel von Association durch unbewusste Mittelglieder. Dr. W. JERUSALEM. *Philos. Stud.*, x. 323-328. 1894.

Sind die Mittelglieder einer Mittelbaren Association bewusst oder unbewusst? W. WUNDT. *Philos. Stud.*, x. 326-328. 1894.

Dr. Jerusalem reports a case given him by a careful observer. The observer, immersed in work, suddenly saw in imagination a scene of which he had not thought for many years. Tracing the cause he found it to be the unnoticed fragrance of a flower in the room, which flower had been part of the original scene.

Professor Wundt adds a note to this observation in which he argues that the smell of the flower was unnoticed but not unconscious—it was perceived but not 'apperceived.'

J. McK. C.

PLEASURE AND PAIN.

The Psychological Analysis and Physical Basis of Pleasure and Pain.

LIGHTNER WITMER. *The Journal of Nervous and Mental Disease*, April 1894, pp. 209-228.

In view of the prevalence of the theory that pleasure and pain are qualities attributable to all states, but not themselves independent sensations, and especially in view of Mr. Marshall's presentation of this theory, Dr. Witmer shows that this is not the most simple and intelligible doctrine, nor that to which the latest discoveries in physiology point. Our current classifications, that of the five senses, for instance, are not founded primarily on introspection or upon the arrangement of feeling accordings to their inward similarity, but rather these classifications are based on psycho-physical observation, that is, they are groupings of sensations according to the identity of their organs or the continuity of their known external causes. This is the case with heat and cold, and with pleasure and pain. Thus the definition of a sensation may be psycho-physical or introspective; it may mean the whole mental effect of exciting a peripheral organ or it may mean the simplest unanalyzable element of consciousness. Now in both senses pleasure and pain are probably sensations: in the former, because recent discoveries show the existence of pain-nerves and suggest the possibility of nerves for pleasure; in the latter, because in cases of extreme pleasure or pain there is hardly any image present, and because, most perceptions being very complex, the existence of pain or pleasure as elements in them is perfectly compatible with the essential independence of these elements.

G. SANTAYANA.

HARVARD UNIVERSITY.

EMOTION.

Recherches sur les rapports de la sensibilité et de l'émotion. P. SOLLIER.

Revue Philosophique, XXXVII. 241-266. 1894.

Prof. James' Theory of Emotion. D. IRONS. *Mind*, N. S. III. 77-97. 1894.

Both the above articles deal with James' theory of emotion. The first seeks to confirm it by experimental evidence, the latter subjects it to a searching and, in the author's judgment, annihilating criticism.

The opening of Dr. Sollier's paper is not calculated to inspire confidence. In the very first paragraph he mixes up the dates at which James and Lange first published their theories of emotion, makes James, who is certainly no pedant, deny that the objective corporeal phenomena which invariably accompany the emotion are its expression, ambiguously states the theory as being that these phenomena are 'its constituents, not to say its cause,' quite overlooking James' admission that there may be purely cerebral emotion, makes the absurd claim that Lange gave the theory a more physiological and *therefore* more scientific basis "in showing that all the phenomena which constitute the emotion are of two orders, vaso-motor and motor," and attributes to Lange the fanciful opinion that the emotional process has its seat in the medulla oblongata. The sequel takes as its point of departure James' suggestion as to what would afford proof positive of his theory, namely, a subject perfectly anæsthetic inside and out who should manifest all the bodily phenomena which usually accompany an emotion without feeling it. But even here Sollier does not report correctly. He makes James say that such cases of absolute anæsthesia are impossible because they would involve death; what James does say is that such cases are extremely hard to find. Parenthetically, however, it may be remarked that James' idea of a test case contains a manifest contradiction. If a subject were totally anæsthetic, an emotion-inspiring object could not affect him at all, much less would he be able to give an account of his experience. The anæsthesia would be necessarily limited to muscular and tactile anæsthesia. But the conditions can be partially realized. There is the well-known case of Strümpell's boy, which, however, as far as the evidence went, was unfavorable to the theory. Sollier refers to a recent case—that of a man who spontaneously developed profound visceral and peripheral anæsthesia and at the same time and to the same degree emotional apathy. It is to be observed, however, that this man still has feelings of distress on waking, of a moving sort on seeing his wife, and of fear lest his daughter should die, a fear so great that he feels that if she were to die he would not survive the shock. Sollier takes no account of these facts, but simply dwells on the parallel development of the general insensibility and the general apathy. His questions in examining the man do not appear to have been particularly searching nor his methods of investigation very exact. He does not, for instance, attempt to discover what bodily processes may have accompanied the afore-mentioned emotions; indeed, to have done so would have involved him in a certain perplexity. For if, on the one hand, none of the ordinary phenomena had been discoverable, it would have proved conclusively that they at any rate could not have been 'constituents,

not to say causes,' of the emotions; if, on the other hand, they had been found to be present, the same conclusion would have been necessary, provided the body were really anæsthetic. Nor is the slightest consideration given to the facts which indicate a central origin, or at least a central element in the origin, of the emotion; and yet, with most of the special senses profoundly and all to some degree impaired, with the apperceptive processes so reduced that the significance of many things is no longer noted, and with the functions of the brain so generally restricted that there is often no thought at all of anything, enough is surely suggested pointing to this view that was worthy of more thorough investigation.

Similar remarks apply to Sollier's treatment of the two hysterical subjects in whom he developed visceral and peripheral anæsthesias by suggestion. He concludes as the results of his experiments that the suppression of general and sensorial sensibility involves the abolition of emotivity to the same degree as the anæsthesia, that the phenomena of muscular and special sensibility play in this relation but a very small part, and that in all cases the state of emotion depends directly on the sensibility and not on motor phenomena as such. Now these results are certainly important, and in themselves are not surprising. But one cannot help entertaining a certain reserve towards them as general principles when one considers the small number of the cases, the extremely limited range of the emotions sought to be excited (really only two, joy and grief), the laxness of procedure, and the conflicting state of the evidence. As to the last points: the patients say in their anæsthetic state that they feel nothing and yet they also say that their members feel numb and cold; one of them declares that, in a certain experiment, she felt nothing, having just previously declared that she felt an impulse to laugh; and the same patient is supposed to be free of emotion when, as she states, she derives no pleasure from the knowledge that her lover adores her, but is only flattered! In spite of the fact that James' ideal case supposes, along with the anæsthesia, the full play of the emotional expression, little is done to determine the latter and its relation to the emotional process; the pneumograph is used but not the plethysmograph, and we are never sure that we have the full emotional expression in any case, the indications being rather the other way. We have, indeed, a case like this: M., profoundly hypnotized and 'insensible,' is told that her father is dead. There is a deep respiration and a very slight contraction of the face as though she is about to cry. But she feels, we are told, no emotion, but only physical sensations of having been struck in the head and stomach. But these are very far from being all the normal expressions of grief; and so far as they are such expressions, they suggest that there are ways of feeling

them without feeling emotion, and here, of course, the condition of bodily anaesthesia is wanting. But even if it should prove true that bodily and especially organic sensibility is essential to emotion, it is still far from being proved that emotion is constituted solely by feelings of somatic resonance; it might still be that the bodily phenomena were produced, as Sollier once inadvertently writes, 'under the influence of emotion,' or at least of some form of cerebrally-originated feeling. To prove the contrary, it would be necessary to show either that no such thing exists as cerebral sensibility, or that, if it does exist, it is independent of general bodily sensibility.

The theory of the mechanism of emotion which rests on the identification of kinaesthetic and cœnaesthetic sensations and centres, and which assumes a cortical vaso-motor centre in the region of the sensorial centres as the seat of the emotional process, can only be mentioned.

The thorough overhauling which James' theory meets with in the paper of Mr. Irons proves this much, at any rate: that, in order to avoid natural misunderstandings, both the theory itself and some of the arguments to support it require to be carefully restated. It is contended, and strongly, that the evidence to show that bodily changes follow the perception of the exciting fact without intervening emotion, *in cases where there is any emotional experience at all*, is meagre and questionable; and against the second part of the theory, to wit, that the emotion is the feeling of the bodily changes, it is urged that though consciousness of bodily disturbance almost always involves emotion, in and for itself, this consciousness is not emotional at all. What James describes as the vital point of the whole theory, the challenge to imagine an emotion after abstracting from the bodily feelings, is met by the logical rejoinder that inseparable correlates are not indistinguishable, and by the appeal to introspection, which very well distinguishes between organic sensations, the consciousness of bodily affections, and emotion as feeling-attitude towards an object. James himself bears indirect testimony to this view in describing doubt and belief by this very term 'psychic attitude,' and against the universality of his theory by teaching that æsthetic emotion is *directly* sensational. As to this last point, Irons remarks acutely that in replying to the objection that the theory breaks down, James' answer is an insistence that it does (see *Principles of Psychology*, II. 468). Then there is the experimental evidence of Strümpell's boy, the force of which, it is urged, is not to be set aside by an abstract doubt. The psychological principles on which the theory rests are thoroughly sensationalistic. The true view can only be given by a spiritualistic psychology. The self reacts as a whole to the stimulus. Emotion is essentially a purely psychical process with bodily

results. It is not mere pleasure or pain, but a species of feeling-attitude, excited by the object but mediated by the activity of the self, and differing from other feeling-attitudes, such as moods and temperaments, interest, expectation and indifference, by its felt diffusedness and strength.

There is much in this contention to agree with, especially the recognition of the unique character of emotion as neither an aggregate of pleasure-pain feelings nor mere sensation of qualities, but as feeling-in-regard-to an object—it might have been added, with impulsive tendency. But we cannot help suspecting that the controlling idea in the author's criticism of Prof. James rests on a misconception, a misconception for which the latter is partly responsible. Irons seems to think that James denies the reality of emotion altogether, that he reduces it to the bare consciousness of the objective bodily phenomena as such. Hence he substitutes for the phrase 'feeling of the bodily changes as they arise' the phrase 'consciousness of bodily changes,' and speaks of 'this *feelingless* theory of emotion.' But is this necessarily James' meaning? Emotion is what it is, however it may be constituted; and just as the sensation of color might be spoken of as the feeling of and as constituted by the minute molecular changes occurring in the visual centre, notwithstanding that it is not the objective consciousness of these cerebral phenomena at all, so emotion might be spoken of as the feeling of physical disturbances in the body in the sense that it is a form of the subjective reflex in which those disturbances are manifested to consciousness. It would not be necessary then to deny the distinction between emotion and the sense-feeling of the bodily states themselves. We should rather distinguish two aspects in the 'feeling' of the bodily phenomena; for instance, (1) sensations with affective tone entering into the painful consciousness of the fact that the heart flutters, the breathing is hindered, etc., and (2) the state of fear arising from the synthesis of these and other connected sensations, the feeling-impulse attitude of the subject towards the object. Whether any such distinction as this lurks hidden beneath James' conveniently ambiguous terminology we do not know; only without it it hardly seems possible to do justice to the experienced facts. To say that bodily resonance is essential to emotion is one thing, although Prof. James does not insist even on that; but to say that emotion is nothing but the feeling of the bodily resonance is quite another, and is contradicted by the only possible authority in this matter, introspective consciousness, if by 'feeling' be meant nothing more than consciousness of a distinguishable aggregate of sensations in muscles and viscera and on the surface of the body. Physiological psychology is not called on to deny the 'spiritual element' in experience, but to explain the conditions of its

genesis; and the question here is whether the 'psychic attitude' of feeling and impulse which we designate as an emotion arises as a subjective reaction on the cerebral excitement connected with the perception of an 'object' prior to the subsequent changes in the rest of the body, or whether it is one form of the consciousness of those changes themselves. Perhaps it may be necessary to more carefully distinguish than has sometimes happened the different phases in the development of an emotion. On the other hand, the courage of a so-called spiritualistic psychology is to be more wondered at than imitated, which at the present day insists, as Mr. Irons appears to do, that there are certain elements in states of consciousness which represent a purely subjective reaction without any corresponding bodily processes whatever.

Le sentiment et l'analyse. F. RAUH. *Revue Philosophique*, xxxvii. 499-513.

Analysis sometimes multiplies ('Egotists') and sometimes suppresses or attenuates feeling (Spinoza); sometimes it 'troubles' feelings in their development (Bourget) and sometimes it enters in a peculiar way into their normal development (Rauh). To account for these different effects, M. Rauh makes use of Paulhan's and Fouillée's theory of psychological dynamism: the feelings are regarded as quasi-independent entities. The dynamism, however, is not only or mainly 'pure,' but also and more generally 'qualitative,' and in certain important respects 'intellectual.' In other words, feelings appear now as brute forces simply, measured solely by their effects and their duration, now as brute forces but of specific quality, and now as unconscious intelligent forces. In the first case, analysis may serve simply to add to the original pleasure the pleasures derived from consideration of the details, a passion by its sheer strength may suppress the intellectual pleasures or the intellectual activity may calm or suppress the emotion. In the second case, the feelings influence one another as like or unlike according to their several affinities or repulsions, and to these are referred the changes in the original feeling consequent on the new feelings aroused by distraction of the attention in reflection. The kind and variety of the changes thus brought about require the further supposition that each feeling tends to assimilate from among the elements of consciousness present at a given time those that are favorable to it and to repel those that are opposed, the promptness and completeness with which it does this being dependent on its strength. This leads, in the third place, to the recognition of the teleology of feeling: emotion, when it has attained a definite character, tends to select and reject with reference to its own satisfaction. Here feeling appears not

only as a force, but as an instinctively intelligent force, attaining its end not merely by its strength, but more particularly by its tact in selection. This is the cardinal point in the theory, from which the practical conclusion follows that persons with the mania of analysis are maladroits. Passion, in order to master the spirit of analysis, must absorb it.

To the objection that the language used in the foregoing theory is metaphorical, the author replies that a metaphor which serves to explain the facts comes near to being a scientific conception. However, the dynamic conception is not, in his judgment, adequate to cover all cases. "The feelings can no more be regarded from a single point of view than the other facts of mind." There are certain anomalous phenomena, such as Descartes' predilection for squint eyes, which seem to represent, not tendencies in process of evolution, but absolutely rigid facts, without even the history or the capacity for modification of a habit, and these are best treated from the point of view of 'English phenomenism.'

On the Nature of Æsthetic Emotion. BERNARD BOSANQUET. *Mind*, N. S. vol. III. 153-166. April, 1894.

Most English writers have inclined to regard æsthetic enjoyment as an aggregate of pleasurable feelings passively received from the beautiful object; Bosanquet holds that the mind of one who, in contemplating the object, goes beyond the first indiscriminating impression which finds utterance in the exclamation 'How beautiful!' is essentially active and tends to assume the attitude of the 'maker.' His theory is that the central characteristic of æsthetic emotion is an aspect of the 'expressiveness' which is the central characteristic of æsthetic presentation, and that the two aspects, the presentational and the emotional, being strictly correlative, we have no need to violate a logical principle by ever assuming plurality of alternative causes for the 'circle of effects' known as beauty. 'Æsthetic emotion first arises in and is constituted by expression for expression's sake'—the phrase marking the generally accepted distinction between the æsthetic and other points of view. Emotions aroused by mere associations irrelevant to the context of the universal pervading the presented content are not properly æsthetic at all. In this connection there is a sharp criticism of the tendency of associationist æsthetics to obliterate the line between what is beautiful and what is personally interesting and to count among the æsthetic feelings the suggestions of mere dumb gratification of the senses. No truly æsthetic emotion, not even where it concerns particular sense-elements,—for instance, a particular

color,—is ever quite dumb ; it is always felt as in a sort an articulate utterance.

H. N. GARDINER.

SMITH COLLEGE.

Sur les idées générales. G. MARCHESINI. Rev. Philos., XVIII. 489-498. 1893.

A chapter from an unpublished work on 'Psychological Monism.' The author takes the position that the source of every mental fact is a datum of the senses, and every abstract form "merely a logical aspect which the sense-datum and the association of sensations acquire by means of analysis." "An idea is a sensation which has left a mental residue capable of elaboration." Through this latent persistence of sense-data (memory) we come to recognize that qualities belonging to one concrete belong to several concretes ; and this resemblance may pass over into a general concept, i.e., into the idea of the possibility of generalizing qualities which are perceived in succession. The rest of the article is an elaboration of this position and a refutation of the Kantian view of *a priori* mental forms.

H. C. WARREN.

PRINCETON COLLEGE.

NEW BOOKS.

Locke's Essay concerning the Human Understanding. A. C. FRASER.

Oxford, The Clarendon Press, 1894. Pp. cxi + 535, 495.

The Psychic Factor : an Outline of Psychology. C. VAN NORDEN. New

York, D. Appleton & Co., 1884. Pp. vii + 223.

Psychologie des grands calculateurs et joueurs d'échecs. A. BINET. Paris,

Hachette et Cie., 1894. Pp. viii + 364.

Versuch einer Theorie der Existentialurteile. H. CORNELIUS. Munich,

Rieger, 1894. Pp. 104.

La teoria sulla formazione naturale dell'istinto. F. MASCI. Naples,

1893. Pp. 120.

L'année philosophique. Quatrième année (1893). Paris, Alcan, 1894.

Pp. 316.

La psychologie de l'amour. G. DANVILLE. Paris, Alcan, 1894. Pp.

iii + 169.

The Elements of Metaphysics. P. DEUSSEN. Translated by C. M.

DUFF. London and New York, Macmillan & Co., 1894. Pp.

xxiv + 337.

An Illustrated Dictionary of Medicine, Biology, and Allied Sciences.

G. M. GOULD. Philadelphia, Blakiston, Son & Co., 1894. Pp.

xvi + 1633.

Our Notions of Number and Space. HERBERT NICHOLS. Boston,

Ginn & Co., 1894. Pp. vi + 201.

NOTES.

Professor G. M. DUNCAN has been promoted to a full Professorship of Philosophy in Yale University.

Mr. GEORGE H. MEAD and Mr. JAMES R. ANGELL have been appointed assistant professors in the University of Chicago.

Mr. F. C. FRENCH of Colgate University has been appointed Professor of Philosophy at Vassar College, while President TAYLOR still retains temporarily the Professorship of Ethics.

Miss E. SEBRING, M.A. (Columbia), has been appointed Instructor in Psychology in the Teachers' College, New York.

Professor JONES of St. Andrews has been called to the chair in Philosophy at Glasgow vacated by Professor EDWARD CAIRD.

A fourth edition of the great work of VOLKMANN, *Lehrbuch der Psychologie*, is announced. It is to appear in two volumes—one of which may be expected in 1894 and also in *Hefte* at short intervals. It may now be ordered in either form (Gotha, Cöthen). The editor, Professor CORNELIUS, supplies the new matter, which will include a thorough revision of the bibliographical sections to date.

Messrs Macmillan & Co. announce the publication of a *Columbia University Biological Series* edited by Professor OSBORN. Two volumes of the series are to be issued in September: *From the Greeks to Darwin* by Professor OSBORN and *Amphioxus and the Ancestry of the Vertebrates* by Mr. ARTHUR WILLEY.

All communications for the editors of THE PSYCHOLOGICAL REVIEW, together with books, reprints, etc., intended for review, should be sent during the year beginning Oct. 1st, 1894, to Professor J. Mark Baldwin, Princeton, New Jersey.